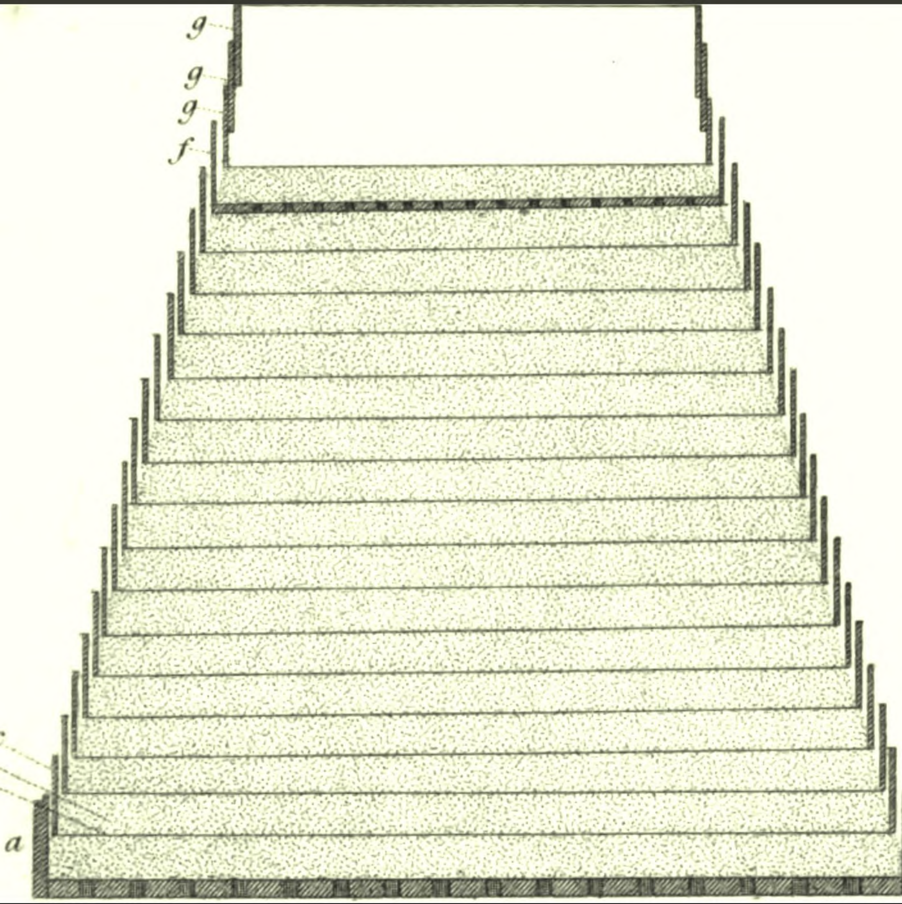

A Journal of natural philosophy, chemistry and the arts

William Nicholson

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A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

N.S.

VOL. XI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

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Of

PREFACE.

Of the Engravings, the Subjects are, 1. A very simple and perfectly safe Gun Lock, by Mr. Dodd. 2. The framing of a Field Gate with a considerable Accession of Strength, by Mr. Waistell. 3. A Compensation Curb for a Time Piece, by Mr. Scott. 5, 6. Plans and Sections of the Bavarian Salt Works. 7. Captain Brodie's Method of connecting Iron Bars, and coating them with Lead. 8. Mr. Rawlinson's Mill for grinding Colours. 9. Apparatus for grinding Indigo and other dry Materials. 10. Method of banking the Balance of a Time Piece, by Mr. Hardy. 11. A Pyrometer for meaſuring the Heat of a Furnace, by Mr. J. G. F. Schmidt. 12. A Portable Steam Engine, by Mr. Murray. 13. Apparatus for making the Gaseous Oxide of Carbon, by Mr. Baruel. 14. A Topographical Plan by Mr. Churchman, in which the Elevations and Depressions of the Surface of the Earth, are by a new Method correctly delienated. 15. Mr. Gregory's Apparatus for measuring the Power of Horſes. 16. The American Borer. 17. Diagram illustrative of Optics, by Mr. Ezekiel Walker. 18. Mr. Sepping's Blocks for supporting and suspending Ships. 19. Count Rumford's Apparatus for illustrating his Experiments in Proof, that the greatest Density of Water is higher than the freezing Point. 20. A Valve for Steam Engines, by the Chevalier Edelcrantz. 21. Figures and Diagrams, by Edward Pigott, Eſq. to illustrate the Causes of Periodical Decrease in Diminution of Light, in a Star in Sobieſki's Shield. 22. A Press for Botanical Specimens, by Mr. Harrison.

Soho Square, London, September 1, 1805.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1805.

ARTICLE I.

Letter from Mr. THOMAS REID, *on the Construction of Time-keeping Machines.*

To Mr. NICHOLSON.

SIR,

IN your interesting and useful Journal of December last, I was glad to see the improvement of compensation pendulums for astronomical clocks, so zealously taken up by such an able hand, as that of Mr. Edward Troughton's.

Compensation pendulum of Mr. Edward Troughton.

But an excellent clock of this sort becomes so very valuable and necessary an appendage in an observatory, to those astronomical instruments with which he is so happily engaged in daily constructing and improving; that he must more readily see their advantage than even those whose business it is to make such clocks. If Mr. Berthoud, a celebrated author on every part that regards the improvement of time-keeping machines, is correct, it would appear, that the steel wires of Mr. Troughton's pendulum must be too slender. Mr. Berthoud, by his experiments, saw, that there was a certain strength of materials necessary, in order to render the compensation complete having found, that on the pendulum rods (if too small) being lengthened by heat, the contraction by cold would not bring

Probability from Berthoud's experiments that the steel wires may be too slight.

bring the ball again quite up to the place where it ſet out from when the heat was firſt applied, and this with a ball of a moderate weight; no doubt the weight of the ball may be made ſubſervient to any ſize of wires.

Mr. Troughton ſeems to have attended to this.

Whether Mr. Troughton has attended to theſe circumſtances, I know not, but ſuſpect from his propoſing, yet, to make ſome ſort of pyrometrical apparatus for the further proving of his pendulums, that the complete compenſation has not been fully aſcertained, only in ſo far as regards the calculation of the relative expanſions of braſs and ſteel.

The improvement of clocks, and time-keeping machines of every deſcription, more particularly thoſe deſtined for aſtronomical purpoſes, is a ſubject that has not a little engaged my time and attention.

The pendulum of Ludlam with a wooden rod examined.

There is a pendulum, having a wooden rod, the conſtruction of which is deſcribed with great neatneſs, perſpicuity, and mechanical knowledge, by its author, (Mr. Ludlam, late an eminent profeſſor at Cambridge) in his eſſays, and recommended by him, *who was no mean judge*, as one of the beſt in almoſt every reſpect, particularly in ſo far as regards the impulſe from the clock taking place through the middle line or centre of the rod, to be thence communicated in the ſame line to that of the ball, that hence no circumgyratory motion ſhould take place. Now, although the principle ſet out with *here*, ſeems completely adapted to prevent this ſort of motion, it will be found on trial, as I did, that of all pendulums yet made, it is the moſt liable to generate this very ſort of motion.

It is liable to a ſide oſcillation of the ball round the rod as an axis.

The ball being the middle fruſtum of a globe, a form whoſe matter is much ſpread out from the centre to the edge, and having a large hole, for the rod to paſs through; *this* taking away much of the matter from the centre, tends much more than the lenticular form, to produce the motion Mr. Ludlam wiſhed to avoid. Another great fault of this pendulum, is, that of putting in ſcrews through the wooden rod, to clip the flat part from the crutch; now in changes of weather from moiſt to dry, or by heat and cold, theſe ſcrews will accordingly be found, ſometimes to pinch the flat part of the crutch, and at other times, to leave it at liberty, or even to allow it to have a conſiderable degree of ſhake between the ſcrew points: hence will ariſe very different degrees of impulſe communicated to the pendulum ball. Wood has a very ſenſible latitudinary

The ſcrews which are acted on by the crutch do not preſerve a conſtant diſtance,

titudinary

titudinary alteration, by the effects of heat and cold, or by dry and moist, yet these effects on its length are, rather but very imperceptible, or at least, are, in so small a degree, as have not been well ascertained, to what extent they are; even by those who have made experiments with it on the pyrometer.

because wood varies across the grain by temperature and moisture, &c.

Mr. Berthoud condemns wood as being unfit for pendulum rods, and although he, and others have given tables of the expansion of various materials, yet none of them have condescended to say, what were the effects of heat and cold on wood of any sort.

The end expansion of wood yet unknown.

I am well convinced, that a pendulum may be so fitted up with a wooden rod, as to perform with such a degree of correctness, that it would be a very difficult matter to say, whether it, or the best compensation pendulum yet constructed, when both comparatively tried, was the nearest to accurate measuring of time.

Wooden pendulums seem equal to the compound pendulums,

There has been one circumstance attending all those pendulums fitted up with a wooden rod, that their errors have been imputed to the rod, when in fact, they ought to have been imputed to that of the ball, and these errors have arisen from the manner by which the ball is hung on the rod, resting on its lower edge on the regulating nut; and lead having a considerable degree of expansion, clocks having such pendulums have been found, by those who attended properly to their going, to have gone constantly faster in summer than in winter. Let the ball therefore be hung by its centre on the rod, and a much greater degree of accuracy in time-keeping will be seen to follow.

but are often ill constructed.

In consequence of my trials with Mr. Ludlam's pendulums, they were found to be extremely troublesome to put on beat, from their strong tendency to this gyratory sort of motion, it being some while, before they would come to move steadily; I hit not only on a method of putting a clock, as it were mechanically on beat, (the common way being by the ear) but was led to think on a way of constructing a pendulum, in which this gyratory motion could hardly take place, even although the pendulum should be but indifferently fitted up. This last was by following a method quite the reverse to that of Mr. Ludlam's, in making the pendulum ball, which I made in the usual or lenticular form, and in order that it should have as much of its matter preserved at the centre, there were two

Improved pendulum.

steel wires put through the ball, passing parallel to each other, and each put a little to one side of the centre, through which pendulum rods are usually made to pass, and when the rod is wood, it necessarily takes away much of the matter from the centre of the ball.

A pendulum of this sort of mine, and which has a degree of compensation in it, I made to a clock, which my brother got, and which he has at his house, No. 31, Rosamon Street, Clerkenwell.

The experiment applied to watches, &c.

I mentioned this mode of putting clocks mechanically on beat, to my ingenious friend, Mr. Pennington, who has since very happily applied the same successfully in his practice to watches, &c.

Pendulum for regulating the striking part of clocks; not new.

I see you have mentioned in your Journal of July last, the application of a pendulum to regulate the striking part of clocks, from the Society for encouraging Arts, &c. having given a premium for it to Mr. Massy.

This is not a new thing. Mr. Berthoud mentions it as his invention, and you will see a drawing, and the description of it, in his *Essai sur l'Horlogerie*, published in 1763. Julien Le Roy, in my humble opinion, is certainly intitled to the merit of it, as it appears to me, that Berthoud has taken the idea of it from Le Roy's method of regulating the striking train of his repeating watches, which he *invented*, and applied to them about the year 1754.

The crank 'scapement, not new;

There was a premium given also in 1799, by the honourable society above mentioned, for a new 'scapement by Mr. Goodrich*; now this 'scapement was made prior to the year 1740, and invented by the Abbé Soumille; and another nearly of the same sort was made at Rome before that period, as may be seen in the collection published by Thiout, in the year 1741. Surely nobody would think of adopting such a 'scapement as this, whose principle seems to be that of depriving the pendulum of the most valuable property it possesses, viz. that of having the liberty to operate freely under the influence of gravitation. This 'scapement keeping the pendulum, as it were in leading strings.

and bad.

I am surprised that none of the members of this honourable and useful society, should not have known, that these

† See Philos. Journal, quarto series, III. 342, 416. It is a crank.—N.

things

things for which premiums have been adjudged were not new, perhaps they were neverthelefs *fo*, to thofe to whom the premiums were adjudged.

Among Thiout's collection may be feen a 'fcapement, which he gives to the ingenious Dutertre, about the year 1724. Peter Le Roy gave into the Royal Academy of Arts and Sciences in the year 1727, a defcription of the fame 'fcapement which Dutertre claimed, or pretended to fay was his invention. The miftake lays with Thiout, for Dutertre's 'fcapement is an improvement of a very old one, ufed by the Germans in large clocks, perhaps long before the year 1600, though neither the author of it, nor the time *when*, can now be traced.

A 'fcapement made long ago

However it was before Dr. Hook's time, who invented one of the fame kind before 1658. I would not have entered fo minutely into this difcuffion, but to fhew the progrefs of the duplex, which in its prefent form, was firft made fo by Peter Le Roy, who afterwards gave it up for a bad one; yet it ought to be allowed that that of Dutertre's muft have led him very eafily to it. So much for the duplex 'fcapement, fo called by the workmen, and now in fuch general ufe. A celebrated Philofopher in the fupplement to the Edinburgh Encyclopedia, under the article watch work, has given it a French furname, that of Dupleix, for what reafon, I know not.

leading to the duplex 'fcapement which was firft made by Peter Le Roy.

Thiout has given alfo among the number, a 'fcapement of his own, a fort of detached one, and which may be confidered as the foundation of the detached one of the prefent day, now fo much improved, and of fuch general ufe in all our pocket and box chronometers: indeed it feems a fcapement indifpenfably neceffary for thefe purpofes. Yet Berthoud in his famous time-keeper, No. 8, ufed a very different one from that of the detached fort.

The origin of the common detached 'fcapement afcribed to Thiout.

I am,

Sir, with much efteem, Your's,

THOMAS REID.

Edinburgh, 25*th March*, 1805.

An

II.

An Analysis of the Magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By CHARLES HATCHETT, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

(Concluded from page 276.)

§ VII.

Whether the artificial pyrites with minimum of sulphur be, like the natural, magnetical.

SO far, therefore, as can be proved by similarity in chemical properties and analysis, the magnetical pyrites is indisputably a natural sulphuret, completely the same with that which till now has been only known as an artificial product; but, that the mind may be perfectly satisfied, another question must be solved, namely, how far do they accord in receiving and retaining the property of magnetism? common pyrites do not appear to affect the magnetic needle, or, if some of them slightly act by attraction, (which however I never could perceive, nor recollect to have read in works expressly relating to magnetism,) yet they do not possess, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic state, and in a considerable proportion, the destruction of this characteristic property of metallic iron, must be ascribed to the other ingredient, sulphur.

The artificial compound is not a mere mixture.

But we have lately seen, that a natural combination of iron with 36.50 or 37 *per cent.* of sulphur, is in possession of all the properties supposed hitherto to appertain (in any marked degree) almost exclusively to the well known magnetic iron ore; and that the combination alluded to is strictly chemical, and not (as at first might have been imagined) a mixture of particles of magnetic iron ore with common pyrites *.

The compound directly formed at red heat.

This is certainly very remarkable; and it induced me to examine the effects produced by sulphur, on the capacity of metallic iron for receiving and retaining the magnetic properties. I therefore prepared some sulphuret of iron, by adding a large quantity of sulphur to fine iron wire, in a moderate red heat.

* This has been sufficiently proved, by the facts which have been stated; I shall however add, that upon digesting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

The

The internal colour and lustre of the product, were not very unlike those of the magnetical pyrites; and, after the mass had been placed during a few hours between magnetical bars, I found that it possessed so strong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and although several weeks have elapsed since it has been removed from the magnetical bars, it still retains its power, with little diminution; like the magnetical pyrites, however, in its natural state, it is not sufficiently powerful to attract and take up iron filings. resembled common pyrites; but was capable of magnetism.

But this sulphuret did not contain so much sulphur as the magnetical pyrites; I therefore mixed some of it, reduced to powder, with a large quantity of sulphur, and subjected it to distillation in a retort, which was at length heated until the intire bulb became red. It contained less sulphur than the magnetical pyrites. More sulphur added and low ignition.

The sulphuret, by this operation, had assumed very much the appearance of the powder of common pyrites, in respect to colour; but, in its chemical properties, such as solubility in muriatic acid, with the production of sulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with prussiate of potash and with ammonia, it perfectly resembled the magnetical pyrites. Moreover, by analysis, it was found to consist of 35 parts of sulphur and 65 of iron; and although (being in a pulverulent state) its power, as to receiving and retaining the magnetic property, could not so easily be examined, yet, by being powerfully attracted by the magnet, with some other circumstances, there was every reason to conclude, that in this respect also it was not inferior. It was attractable by the magnet.

Another proportion of sulphuret was formed, as above described; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

It is certain, therefore, that when a quantity of sulphur equal to 35 or 37 *per cent.* is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, so that the mass acts in every respect as a permanent magnet. Hence 36 or 37 per cent. does not prevent iron from becoming a magnet.

Black oxide of iron, by one operation, does not appear to combine with sulphur so readily as iron filings; a second operation, however, converts it into a sulphuret, very much resembling that which has just been described, including the chemical Black oxide of iron combines with sulphur less readily.

chemical as well as the magnetical properties; but, undoubtedly, by these processes, it is progressively converted, perfectly or very nearly, into the metallic state.

Iron combined with a larger proportion of oxygen, such as the fine gray specular iron from Sweden, will not form a sulphuret by the direct application of sulphur, in one operation; although it becomes of a dark brown colour, partly iridescent, and is moderately attracted by a magnet.

Magnetical pyrites combined with 9 *per cent.* more of sulphur,

Fifty grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of sulphur, were distilled in a retort, until the bulb became moderately red-hot. After the distillation, the pyrites weighed 54.50; consequently, the addition of sulphur was 9 *per cent.* making the total = 45.50 or 46 *per cent.* The powder was become greenish-yellow, very like that of the common pyrites: it did not afford any sulphuretted hydrogen, when digested in muriatic acid; but it nevertheless was partially dissolved, and the solution, when examined by prussiate of potash, and by ammonia, was not different from that of the crude magnetical pyrites.

—was still attracted by the magnet.

The powder which had been distilled with sulphur, and which had thus received an addition of 9 *per cent.* to its original quantity, *was still capable of being completely taken up by a magnet.*

Iron ceases to be acted on by the magnet when the dose of sulphur is at some point between 46 and 52 *per cent.*

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a considerable proportion of sulphur, is not only still capable of receiving the magnetic property, but is also thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above stated) with 45 or 46 *per cent.* of sulphur, is capable of being taken up by a magnet, whilst iron combined with 52 *per cent.* or more, of sulphur, (although likewise in the metallic state,) does not sensibly affect the magnetic needle; and hence, small as the difference may appear, there is reason to conclude, that the capacity of iron for magnetic action is destroyed by a certain proportion of sulphur, the effects of which, although little if at all sensible at 46 *per cent.* are yet nearly or quite absolute, in this destruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of sulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.

As

As carbon acts on soft iron, (which, although it most readily receives the magnetic influence, is unable to retain it so as to become a magnet, without the addition of a certain proportion of carbon, by which it is rendered hard and brittle, or, in other words, is converted into steel, so, in like manner, does sulphur seem to act; for it has been proved, by the preceding experiments, that the brittle mass formed by the union of a certain proportion of this substance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet.

Sulphur acts on iron like carbon in causing it to contain magnetism,

This remarkable coincidence, in the effects produced on iron by carbon and sulphur, induced me to try the effects of phosphorus; and my hope of success was increased by the remark of Mr. Pelletier, who says, that "the phosphuret of iron is attracted by the magnet;" * and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was desirous to examine what might be the power, in this respect, of phosphuret of iron.

—and so likewise does phosphorus.

I therefore prepared a quantity of phosphuret of iron, in the direct way, *viz.* by adding phosphorus, cut into small pieces, to fine iron wire made moderately red-hot in a crucible. The usual phenomena took place, such as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a striated grain, extremely brittle, hard, and completely converted into a phosphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largest pieces, during a few hours, between magnetical bars, I had the pleasure to find that these had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and small pieces, about half an inch in length, of fine harpsichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot discover any diminution of the power which had thus been communicated to them.

Experiment. Phosphuret of iron was made;

—and by the touch formed a powerful magnet.

The three inflammable substances, *carbon*, *sulphur*, and *phosphorus*, which, by their chemical effects on iron, in many respects resemble each other, have now therefore been proved

* "Le Phosphure de Fer est attirable a l'aimant." Annales de Chimie, Tome XIII. p. 114.

alike

alike to possess the property of enabling iron to retain the power of magnetism; but I shall consider this more fully in the following section.

§ VIII.

From the whole which has been stated we find,

General results. Magnetic pyrites is a British mineral,

1. That the substance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is also a British mineral, and that, in Caernarvonshire, it forms a vein of considerable extent, breadth, and depth.

—containing about 37 sulphur and 63 iron.

2. That the component ingredients of it are sulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

It differs in its properties from common pyrites which contains more sulphur.

3. That the chemical and other properties of this substance are very different from those of the common martial pyrites, which however are also composed of sulphur and iron, varying in proportion, from 52.15 to 54.34 of sulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of sulphur, and 46.75 of iron; consequently, the difference between the relative proportions, in the composition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

It is identically the same as the artificial sulphuret.

4. That, as the magnetical pyrites agrees in analytical results, as well as in all chemical and other properties, with that sulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the same; and we may conclude, that its proportions are most probably subjected to a certain law, (as Mr. Proust has observed in the case of the artificial sulphuret,) which law, under certain circumstances, and especially during the natural formation of this substance in the humid way, may be supposed to act in an almost invariable manner.

In common pyrites the sulphur predominates.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that sulphur becomes the predominant ingredient, which is variable in quantity, but which, by the present experiments, has not been found to exceed 54.34 *per cent.* a proportion, however, that possibly may be surpassed in other pyrites, which have not as yet been chemically examined.

6. That

6. That iron, when combined naturally or artificially with 35.50 or 37 of sulphur, is not only still capable of receiving the magnetic fluid, but is also rendered capable of retaining it, so as to become in every respect a permanent magnet; and the same may, in a great measure, be inferred respecting iron which has been artificially combined with 45.50 *per cent.* of sulphur. Limits of the sulphur in magnetic pyrites.

7. That beyond this proportion of 45.50 or 46 *per cent.* of sulphur, (in the natural common pyrites,) all susceptibility of the magnetic influence appears to be destroyed; and, although the precise proportion which is capable of producing this effect, has not yet been determined by actual experiment, it is certain that the limits are between 45.50 and 52.15; unless some unknown alteration has taken place in the state of the sulphur, or of the iron in the common martial pyrites. Proportion beyond which the magnetic influence is lost.

8. That, as carbon, when combined in a certain proportion with iron, (forming steel,) enables it to become a permanent magnet, and as a certain proportion of sulphur communicates the same quality to iron, so also were found to be the effects of phosphorus; for the phosphuret of iron, in this respect, was by much the most powerful, at least when considered comparatively with sulphuret of iron. As carbon renders iron tenacious of magnetism, so also do sulphur and phosphorus.

9. And lastly, that as carbon, sulphur, and phosphorus, produce, by their union with iron, many chemical effects of much similarity, so do each of them, when combined with that metal in certain proportions, not only permit it to receive, but also give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called steel, certain sulphurets and phosphurets of iron may be regarded as bodies peculiarly susceptible of strong magnetical impregnation. Conclusion.

Having thus, for the greater perspicuity, reduced the principal facts of this Paper into a concise order, I shall now make some general observations.

Remarks.

It is undoubtedly not a little singular, that a substance like the magnetical pyrites, which, although not common, has been long known to mineralogists, should not hitherto have been chemically examined, especially as mineralogical authors have mentioned the analysis of it as a desideratum. The result of this which I have attempted, proves that it is really deserving of notice; for thus we have ascertained, that the sulphuret of iron hitherto known only as an artificial product, is also formed by The magnetical pyrites is an interesting product.

by nature; and that the composition of this last, agrees with those proportions of the artificial sulphuret which have been stated by Mr. Proust.

No intermediate natural product between the common and the mag. pyrites.

But, from this sulphuret or magnetical pyrites, I have not, by analysis, as yet been able to discover any regular or immediate gradations into the common pyrites; for the least proportion of sulphur in these amounted to 52.15, and the greatest proportion to 54.34; so that, between the magnetical and the common pyrites, the difference is considerable, in the proportions of their component substances, as well as in their physical and chemical properties; whilst the difference which I have hitherto been able to detect in the proportions of some of the common pyrites (very dissimilar in figure, lustre, colour, and hardness,) has only amounted to 2.19.

Remarks on Proust's experiments.

Mr. Proust, in a general way, considers common pyrites to differ from the first sulphuret, or that composed of 60 parts of sulphur and 100 of iron, (= 37.50 *per cent.*) by containing a farther addition of half the above quantity of sulphur, or 90 parts of sulphur and 100 of iron, (= 47.36 *per cent.*) but this opinion he appears to have formed, in consequence of results obtained by synthetical experiments made in the dry way.—Now, when we consider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities most commonly result from alterations in these degrees of heat, it seems to me that we cannot rely, with absolute certainty, on synthetical experiments made in the above way, unless they are corrected, and contrasted with analytical experiments made on the same substances. But it does not appear, from the two memoirs published by Mr. Proust, to which I have so frequently alluded, that that gentleman did more, in respect to analysis, than distil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 *per cent.* of sulphur; and, having observed that the residuum possessed the properties of the sulphuret which has been commonly prepared in laboratories, he concluded that the sulphur obtained from the pyrites, is the excess of that proportion which is requisite to form the sulphuret, the proportions of which, therefore, he by synthesis ascertained to be, as I have above stated, = 37.50 of sulphur, and 62.50 of iron, or 60 of sulphur combined with 100 of iron; and lastly, having formed 318 grains of this sulphuret from 200 grains of iron

iron filings, he diſtilled the ſulphuret with an additional quantity of ſulphur, in an inferior degree of heat, and obtained 378 grains of a ſubſtance which, excepting denſity, was ſimilar to the common martial pyrites.*

He did not make a regular analyſis.

It is however to be regretted, that Mr. Prouſt did not make a regular analyſis of the pyrites of Soria, and of the reſiduum after diſtillation; for (unleſs theſe pyrites are very different from thoſe which I have examined) he would moſt probably have found the proportion of ſulphur greater than that which he has aſſigned to natural pyrites in general. This at leaſt there is great reaſon to ſuppoſe, if we allow that moſt or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of ſulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the reſults of my analyſes; for, inſtead of finding the general proportions to be 47.36 of ſulphur and 52.64 of iron, the mean reſult of theſe analyſes is very nearly the reverſe, being 53.24 of ſulphur and 46.76 of iron.

Prouſt apprehends that pyrites holding the leaſt ſulphur are moſt liable to vitriolization:

Mr. Prouſt is alſo of opinion, that the pyrites which contain the ſmalleſt quantity of ſulphur, are thoſe which are moſt liable to vitriolization; and, on the contrary, that thoſe which contain the largeſt proportion, are the leaſt affected by the air or weather.† This opinion of the learned profeſſor, by no means accords with ſuch obſervations as I have been able to make; for the cubic, dodecaedral, and other regularly cryſtallized pyrites, are liable to oxidizement, ſo as to become what are called hepatic iron ores, but not to vitriolization; whilſt the radiated pyrites (at leaſt thoſe of this country) are by much the moſt ſubject to the latter effect; and therefore, as the reſults of the preceding analyſes ſhow that the cryſtallized pyrites contain leſs ſulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization obſerved in ſome of the pyrites, not ſo much to the proportion, as to the ſtate of the ſulphur in the compound; for I much ſuſpect, that a prediſpoſition to vitriolization, in theſe pyrites, is produced by a ſmall portion of oxygen being previouſly combined with a part, or with the general maſs,

But this diſpoſition moſt probably ariſes from a commencement of oxidation.

* *Journal de Phyſique*, Tome LIV. p. 92.

† *Journal de Phyſique*, Tome LIII. p. 91.

of the fulphur, at the time of the original formation of thefe fubftances, fo that the ftate of the fulphur is tending to that of oxide, and thus the acceffion of a farther addition of oxygen becomes facilitated. We have an example of fimilar effects in phofphorus, when (as is commonly faid) it is half burned, for the purpofe of preparing the phofphorus bottles; and the propenfity to vitriolization, obferved in many of the half-roafted fulphureous ores, appears to me to arife from this caufe, rather than from the mere diminution of the original proportion of fulphur, or the actual immediate converfion of part of it into fulphuric acid; neverthelefs, I offer this opinion, at prefent, only as a probable conjecture, which may be inveftigated by future experiments and obfervations.

The magnetical properties of the fulphuret is a remarkable fact.

The magnetical properties of the fulphuret of iron which forms the principal fubject of this Paper, muft be regarded as a remarkable fact; for I have not found, in the various publications on magnetifm which I have had the means of confulting, even the moft remote hint, that iron when combined with fulphur, is poffeffed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have fuppofed that fulphur annihilated this power in iron, as indeed feems to have been the opinion of mineralogifts, who have never enumerated magnetical attraction amongft the phyfical properties of thofe bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the fulphurets of iron, yet the magnetical property could not with certainty be ftated as inherent in the fulphuret, for, at that time, this fubftance had not been fubjected to a regular chemical analyfis, and the magnetical property might therefore be fufpected to arife from interfperfed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extenfive Treatife on Mineralogy lately publifhed, I cannot find any mention made of the magnetical pyrites, either amongft the fulphurets or amongft the other ores of iron.

Little or fcarcely noticed.

The magnet faid to confift of iron with 10 to 20 oxigen.

In the mineral kingdom, a great variety of fubftances, and even fome of the gems, exert a feeble degree of attraction on the magnetic needle, and fometimes alfo acquire a flight degree of polarity; * but, as this wonderful property has only been

* Cavallo on Magnetifm, page 73.

obferved

observed conspicuously powerful in one species of iron ore, this has been always emphatically called *the Magnet*,* and is said to consist of metallic iron combined with from 10 to 20 *per cent*. of oxygen.

From the facts, however, which have been recently stated, we now find that there is another natural substance, apparently very different from the magnet in chemical composition, but nevertheless approaching very nearly to it in power, which is found in several parts of our globe, and particularly in a province of this kingdom, where it constitutes a vein, running north and south, of a considerable extent, and several yards in width and thickness. The sulphuret is a different kind of magnet.

From the experiments also, which have been made on the artificial preparation of this substance, we find, that it is capable of receiving the magnetic properties when the proportion of sulphur amounts to 37 *per cent*. and is still powerfully attracted when a much larger quantity of sulphur is present. There is, however, some point at which all these effects cease, and this point appears to be, when the sulphur is in some proportion between 45 or 46 and 52 *per cent*. The preceding experiments have also proved, that iron when combined with phosphorus, likewise possesses the power of becoming a magnet to a very remarkable degree; and, by the similarity, in this respect, of the carburet of iron called steel, to the above sulphuret and phosphuret, a very remarkable analogy is established between the effects produced on iron, by carbon, sulphur, and phosphorus. The phosphuret magnet.

Carbon, when combined in a very large proportion with iron, forms the carburet of that metal called plumbago; a brittle substance, insoluble in muriatic acid, and destitute of magnetical properties. But, smaller proportions of carbon, with the same metal, constitute the various carburets included between black cast iron and soft cast steel;† bodies which are more or less Of carburets, plumbago is not magnetical: other steel compounds form powerful magnets.

* In a future Paper, it is my intention to give an account of some comparative analyses of the varieties of this substance.

† "When the carbon exceeds, the compound is carburet of iron "or plumbago: when the iron exceeds, the compound is steel, or "cast iron, in various states, according to the proportion. All "these compounds may be considered as subcarburets of iron."— Thomson's System of Chemistry, Vol. I. p. 165.

Mr.

lefs brittle, foluble in muriatic acid, and more or lefs fufceptible of magnetical impregnation; fome of them form the moft powerful magnets hitherto difcovered.

Sulphur and iron have fimilar habitudes.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almoft or quite infoluble in muriatic acid, and devoid of magnetical properties. Sulphur in fmaller proportions, forms fulphurets which are alfo brittle, but are foluble in muriatic acid, and ftrongly fufceptible of magnetical impregnation.

So likewife phofphorus and iron.

Phofphorus alfo, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; fo that, confidering the great fimilarity which prevails in other refpects, it may not feem rafh to conclude, that phofphorus (like carbon and fulphur,) when combined with iron in a very large proportion, may form a fubftance incapable of becoming magnetical, although, in fmaller proportion, (as we have feen,) it conftitutes compounds which are not only capable of receiving, but alfo of retaining, the magnetical properties, even fo far as, in fome cafes, to feem likely to form magnets of great power; and, fpeaking generally of the carburets, fulphurets, and phofphurets of iron, I have no doubt but that, by accurate experiments, we fhall find that a certain proportion of the ingredients of each, conftitutes a maximum in the magnetical power of thefe three bodies. When this maximum has been afcertained, it would be proper to compare the relative magnetical power of fteel (which hitherto has alone been em-

Mr. Mufhet, in the following Table, exhibits the proportion of charcoal which difappeared, during the converfion of iron to the different varieties of fubcarburet known in commerce.

" Charcoal abforbed.		Refult.
$\frac{1}{120}$	- - -	Soft caft fteel.
$\frac{1}{100}$	- - -	Common caft fteel.
$\frac{1}{90}$	- - -	The fame, but harder.
$\frac{1}{50}$	- - -	The fame, too hard for drawing.
$\frac{1}{25}$	- - -	White caft iron.
$\frac{1}{20}$	- - -	Mottled caft iron.
$\frac{1}{15}$	- - -	Black caft iron.

" When the carbon amounts to about $\frac{1}{60}$ of the whole mafs, the " hardnefs is at the maximum." Thomfon, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 148.

ployed

ployed to form artificial magnets) with that of fulphuret and phofphuret of iron; each being firft examined in the form of a fingle mafs or bar of equal weight, and afterwards in the ftate of compound magnets, formed like the large horfe-fhoe magnets, by the feparate arrangement of an equal number of bars of the fame fubftance in a box of brafs.

The effects of the above compound magnets fhould then be tried againft others, compofed of bars of the three different fubftances, various in number, and in the mode of arrangement; and, laftly, it would be interefting to make a feries of experiments on chemical compounds, formed by uniting different proportions of carbon, fulphur, and phofphorus, with one and the fame mafs of iron. Thefe quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-fulphuro phofphurets, or phofphuro-fulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may alfo, by the inveftigation of their magnetical properties, afford fome curious refults. At any rate, an unexplored field of extenfive refearch appears to be opened, which poffibly may furnifh important additions to the hiftory of magnetifm, a branch of fcience which of late years has been but little augmented, and which, amidft the prefent rapid progrefs of human knowledge, remains immerfed in confiderable obfcurity.

An extenfive field for experiments of intereft and importance.

III.

Extract of a Memoir of Mr. ERMAN, *entitled Obfervations and Doubts concerning Atmofpheric Electricity.*

(Concluded from p. 300, Vol. X.)

AN electrometer furnifhed with a rod three feet in length, and placed in the open air, does not fhew any divergence; but when the bent point of another electrometer, which alfo exhibits no divergence, is moved above the firft, and even when the motion is parallel to the horizon, the leaves of the latter will be feen to diverge negatively, without the fecond giving any fign of electricity.

Two electrometers which do not indicate divergence alone, fhew it when one is paffed above the other.

It is very probable that the effect of thefe vapourous and aqueous meteorological maffes is manifefted at the points of the electrometer,

This effect probably arifes from the divifion of the electricity.

electrometer, by this same action, which depends on the division of the electricity, and this explains the sudden changes which supervene in the positive or negative electric state of the earth: it is not even probable that the clouds possess in themselves a negative electricity, or that the vapours of water should be always in this state. Mr. Erman thinks he can prove that the vapours of water are only negative when the body from which they proceed is insulated; but that they become positive as soon as it is brought into contact with the earth. Rain, after its fall, leaves bodies in the state of *minus* electricity, which is agreeable to the preceding experiments; snow produces this effect so much the better, as it leaves the surrounding air in a state of dryness, which augments its insulating properties.

The positive or negative state of aqueous vapour depends on insulation.

The clouds which have a tendency to rain or snow must necessarily produce their influence on the ground, and it is for this reason that the opposition of the cloud and the ground is so quickly manifested. It would be interesting to explain the complication arising from the variations which the electricity of the earth undergoes, as well with respect to its species as to its intensity, and Mr. Erman is at present engaged in this subject.

Influence of the clouds on the earth.

With respect to the variable degree of the positive divergence of an insulated point fixed in the earth, it may perhaps be attributed to the greater or less conducting quality of the surrounding air: the point, when it is very well insulated from the earth, shows a certain degree of positive charge when the air is insulated so that electricity cannot be communicated to it; it shows zero when the conducting power of the air is nearly equal to the rapidity with which the charge is made; and negative when this power is very strong. These phenomena are thus explained by the same law.

The different phenomena of the divergence of an insulated point explained by the same law.

Mr. Erman has also supposed in his memoir that there is not any quantity of electricity disengaged in the atmosphere; but he does not however assert that this is strictly the case: he has only sought to draw the attention of philosophers to this subject, and to show that these phenomena are equally well explained by the sphere of activity of the electricity.

Probability that there is no free electricity in the atmosphere.

The experiments are very easily made, and the electrometers he uses are very portable; for when the leaves of gold are placed on one side of the cylinder, motion cannot occasion any accident. With respect to the sticks, they may be made of several

Convenience of the instruments.

ſeveral pieces, and be ſcrewed together when they are wanted. Such were the inſtruments Mr. Erman took with him over ſeveral hundred leagues; the ſtick was three lines in diameter at one end, and one line at the other. This philoſopher does not yet know what may be the influence of the different thickneſs of the conductor. One of theſe conductors happened to be of braſs, the other of ſteel; it did not appear to him that the difference of the metal occaſioned any in the reſults. But he purpoſes making new enquiries on this ſubject.

Mr. Erman offers his concluſions with great diffidence: he does not pretend to have formed a new theory, but only to ſtate his doubts on the opinion of thoſe philoſophers who have attributed the phenomena hitherto obſerved to a diſengaged electricity in the atmoſphere. He notices ſeveral other intereſting experiments which he is employed in making upon ſmoke, and particularly on electricity in a vacuum. We ſhall haſten to publiſh the reſults of them as ſoon as they come to hand.

IV.

Deſcription of a Compenſation Curb. By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

INCLOSED I ſend you a ſketch of an inſtrument I have conſtructed, much wanted in the profeſſion of watchmaking, for the purpoſe of publiſhing in your Philoſophical Journal, which I preſume will be of much benefit to the public, and at the ſame time may prevent any other perſon from claiming it. Introductory letter.

I ſhall call it a compenſation curb. The conſtruction of this inſtrument is for the purpoſe of expanding and contracting in the different temperatures, ſo as to counteract the error which the pendulum-ſpring is liable to by the ſmalleſt variation of heat or cold.

The inventions that have hitherto been put in practice for the ſame purpoſe, I beg leave to make a few remarks on.— The compound balance, when carefully made and adjuſted, is certainly a very complete counteracting expanſion, and will anſwer exceedingly well on board a ſhip, if there be no material difference in the denſity of the air; if otherwiſe, the balance

lance being loaded, will have to encounter a confiderable deal more friction, and confequently be impeded in its vibrations: if worn in the pocket, it is alfo liable to error, as exercife will alter its diameter. The compound balance has heretofore got the preference, becaufe artifts have not been able to invent a compenfation curb adjuftable to the exact expanfion required (which, by many experiments, I have proved the inclofed to be fully competent to); therefore, the plain balance having no projections on the furface, muft certainly have the advantage. As I am not in the habit of expreffing my ideas to the public, I hope you will have the goodnefs to rectify any errors in the ftile, and alfo curtail any part which may appear unneceffary to you.

I remain, Sir,

Your much obliged humble fervant,

JAMES SCOTT.

39, *Grafton Street, Dublin.*

The following is a Defcription of the Compenfation Curb.

Defcription of a compenfation curb.

The fteel index, *Pl. II. Fig.* 2, letter A, is for the purpofe of fupporting the curb, which is faftened by a fcrew and fteady pin at R, the circle of which at A is turned with a dovetail, as fhewn at H, and is flit fo as to fnap into the frame-plate, by which means it may be turned, and will carry the curb, fo as to regulate the machine in the common way. D and E are two circles compofed of brafs and fteel foldered together, the outfide of E brafs and the infide fteel; but the outfide of D is fteel and the infide brafs; fo that the one circle expands when the other contracts; by which means the acting part of the curb at C will fhift towards the index with heat, and prevent the vibrations being flower, which the expanfion of P, the regulating fpring and the balance, would otherwife occafion; and on the contrary with cold, it will fhift its pofition nearer the ftud I, which the regulating fpring is pinned to; fo that let it receive heat or cold, the acting part of the curb at C will at all times keep the regulating fpring the exact length, to counteract the expanfion of the balance and pendulum-fpring. V is a piece of fteel, with a notch cut in it to receive the expanfion circles D and E. F is a fcrew for faftening V at any part of the circles; fo that, by ftoving the watch,

5 you

you will immediately ascertain towards what part of the circles you must shift V. If you find its rate slow with heat, you must shift V to lengthen your circles, and if fast, the contrary; and by marking your circles each time you have occasion to shift V, yon will be enabled to adjust the curb to the exact expansion required. W is a loose piece of steel, which fits in the notch of V between the two expansion circles, to keep them fast in their proper positions when screwed by F. S S are two steady pins made fast in the frame-plate, which receive the circle E between them to prevent the action of P, the regulating spring, from affecting C, the curb, by moving it to or from the center during the going of the watch. The two expansion circles require to be made very delicate; it is therefore to be observed by the manufacturer of this instrument, that the brass in each circle is to be the thickness of the steel; so that when the two bodies are soldered together, they will make two thicknesses of the edge of a main-spring of a watch. The larger your watch will admit the diameter of the curb, the better. It must be at least the size of the balance. Description of a compensation curb.

V.

Letter from Mr. Boswell, *in Answer to* An Old Correspondent.

To Mr. NICHOLSON.

SIR, *April* 10, 1805.

THE person signing *your Old Correspondent*, has thrown some very undeserved reflections on my last communication in your Journal. Mr. Boswell vindicates himself from confidence.

In answer to his remarks, I beg leave to observe that the introduction to my paper contains sufficient to justify me fully from his imputations.

I have there asserted no more than that "I have discovered "a method of coming so *near the truth*, that should it turn "out to be in reality *not so exact* as appears to me, yet it promises to be so useful for *common computations* that I am "induced to send it for publication, *if you approve of it.*"

I know

I know not, Sir, how I could have written in any way farther from " announcing my discovery with confidence" than the above, or indeed with more diffidence.

His methods were stated as *nearly approaching the truth.*

n the first place, I state it only as a *near approach* to the exact truth, which both your and his criticisms have fully proved it to be: In the next place, I express my doubt of its being even *so exact* as it appears to me: And thirdly, I have left its publication to depend *on your approbation.*

If this is not enough to remove all suspicion of confidence, I have to add, that the sentence which concludes that subject in the paper, states, that an unperceived error might arise from the smallness of the circles which I used; and besides this you can testify for me, that I wished the paper to be suppressed altogether before publication, when you shewed me that it even wanted an hundredth part of being exact, though with a much greater inaccuracy the matter contained in it would be useful for the purposes to which I stated it might be applied.

As to the proof of the second *fact*, it was not from the " conviction of its obvious accuracy not requiring proof" that I did not insert any, but because I concluded it must be sufficiently obvious after what I had stated of the first, that it was the same sort of *experimental* proof I had used for both: And if any gentleman will try the experiment as I did, he will find I have not misstated the matter.

There may indeed be some little impropriety in using the word *fact* in a popular sense, in any thing like a mathematical statement; but to notice such a trifle with inverted commas, only appears to indicate a spirit of cavilling on the part of your Old Correspondent.

None of the quadratures of the circle are more than approximations.

But with all his precision he has forgot one fact, that puts his computations more on a level with my experiments, which is, that no method has ever yet been discovered of computing with *perfect exactness* the relative proportions of the circumference to the diameter, and of course to the other lines he mentions; and that it is only a far-laboured *approximation* to the truth that has been inserted in the work from whence he has extracted the proportions which he has used; and that therefore what he has " announced with so much confi-" dence," (to use his own words) is not precisely demonstrative truth, but only an approach to it.

I did

I did not ever intend to recommend my method as a perfect and infallible geometrical problem applicable to the more sublime mathematical speculations, but as a more ready, and, permit me to add, more exact way than that in general use, for the humbler purposes of common life, such as measuring round timber, conduit pipes, engine cylinders, &c.

Mr. B.'s methods have practical utility.

I am, &c.

J. WHITLEY BOSWELL.

VI.

Description of an improved Gate for Fields. By Mr. CHARLES WAISTELL *.

DEAR SIR,

THE various methods used in bracing common gates for fields, prove that not one of them is greatly superior to the rest; for, if it was, that method would have been generally adopted. Most gates are loaded with superfluous timber in some of their parts, and are constructed upon such bad principles, that they are frequently broken by their own weight, aided by the concussion of the head against the falling-post; and this, long before any part of the wood has begun to decay. I have for some time given this subject considerable attention, being impressed with the idea, that if common gates could be constructed with less timber, and upon better principles, the saving of timber only would be of national importance; for we have many millions of gates to uphold in Britain, and their numbers are annually increasing. The result of my labours has been the plan which accompanies this letter. Gates made according to it, possess great strength, are very light, and of easy and simple construction. Although uniformity of appearance be not essential in a common gate, yet is worth having when it can be obtained, as in this gate, without additional expense.

Imperfections of common gates.

Great saving in timber if they were made less cumbersome.

New plan.

My gate is made with short, and consequently less valuable, oak or ash timber, than those of the commonest construction; its strength is much greater than any other gate made with a like quantity

Account of the author's improvements.

* Communicated in a letter to Charles Taylor, Esq. Secretary of the Society of Arts, who returned their thanks for the same.

quantity of timber, there being at four diſtant points between the head and the heel, two bars and a brace croſſing each other: and I doubt not that it will be found proportionably more durable: it is, beſides, very eaſy to conſtruct, and requires leſs labour than moſt other common gates. Twenty-nine years ago I deſigned plans for ornamental gates, with ſemi-oval and ſemi-circular braces, and had them executed; the plans were ſent to my friends in various diſtant parts of this kingdom, as alſo to Ireland; and I have the pleaſure to obſerve, that they are become almoſt the only ornamental gate in many parts of England. The plans of them I never publiſhed, although they were prepared for engraving fifteen years ago; and I ſhould be as indifferent about my preſent deſign, of a common field gate, if I did not conceive that its publication would materially benefit the public; the introduction of this form being, I conceive, of ſome national importance, as timber has been lately greatly enhanced in price, and is rapidly on the advance.

This gate was deſigned for the approach to a country reſidence; but for common purpoſes, the wicket on one hand, and the ſhort length of rails on the other, may be omitted. I ſhall thank you, if you will have the goodneſs to lay my plan before your reſpectable Society, of which I have, for many years, had the honour to be a member. And ſhould this plan be approved of, I may probably furniſh ſome deſigns for park gates on an improved conſtruction.

I am, Dear Sir,

Your very humble Servant,

CHARLES WAISTELL.

March 22, 1803.

Mr. Charles Taylor.

Reference to the Engraving of Mr. Waiſtell's Gate.

DIMENSIONS.—*(Plate II. Fig. 1.)*

Deſcription and dimenſions of the new conſtructed field-gate.

The heel of the gate to be about	$3\frac{1}{2}$	inches ſquare.
The head of ditto - -	$2\frac{1}{2}$	by 3 inches.
The top rail or bar - -	$3\frac{1}{2}$	by $1\frac{1}{2}$ inches.
The bottom bar - -	$3\frac{1}{2}$	by $1\frac{1}{4}$ inches.
The bar in the middle of the gate	3	by $1\frac{1}{4}$ inches.
The other bars, and the 4 braces	$2\frac{1}{2}$	by $1\frac{1}{4}$ inches.

Obſervations

Obſervations on its Conſtruction.

The head and heel of the gate may be of oak, and the bars and braces of fir. Narrow and thick bars, when braced as in this deſign, are ſtronger than broad and thin ones, containing the ſame quantity of timber, and they alſo oppoſe a leſs ſurface to the wind. The two points in the heel of the gate, to which the thimbles are faſtened, may be conſidered as firm or fixed points. From theſe points, viz. 1 and 2, two braces to proceed to 4 and 3, in the middle of the bottom and top bars, and being there ſecured, theſe become fixed points, and from theſe two points, viz. 4 and 3, two braces proceed to 5 and 6, fixing thoſe points. The gate is thus doubly braced, viz. from the top of the heel to the top of the head, by means of the braces 1, 4, and 4, 5; and from the bottom of the heel to the bottom of the head, by means of the braces 2, 3, and 3, 6. On each ſide of the gate are two braces, and thoſe parallel to each other. The brace proceeding from the bottom of the heel of the gate, and that which is parallel to it, as alſo the bottom bar, are all ſtrained in the way of compreſſion, and the brace proceeding from the top of the heel, and the other brace which is parallel to it, and alſo the top bar, are all ſtrained in the way of extenſion. The ſtrains in this gate being none of them tranverſe, but all longitudinal, it would ſupport a vaſt weight at its head without having its form altered. The braces all ſerve the double purpoſe of keeping the gate in its true form, and of ſhortening the bearings of the bars, and ſtrengthening them. Few gates have leſs timber in their braces; and perhaps in no other way can a gate be ſo firmly braced with ſo ſmall a quantity of timber.

Deſcription and dimenſions of the new conſtructed field-gate.

At 3, 4, 7, and 8, two braces and a bar of the gate are firmly ſcrewed together by means of iron pins and ſcrew nuts. At the other points, where only one brace croſſes a bar, common gate-nails are uſed.

If, in ſome caſes, a ſtrong top-bar be wanted, to reſiſt the preſſure of heavy cattle, a bar or board, about ſix inches broad, and one inch thick, may be laid with its broad ſide upon the top bar, and fixed thereto by means of the ends of the braces in the middle, and by the heel and head of the gate at the two ends of it. This board will, in this poſition, reſiſt exactly the

ſame

same preſſure as a thick top bar, three inches broad, by four inches deep, although it contain no more than half the timber.

In the ground plan, or horizontal ſection, *Fig.* 7. repreſents a piece of wood, about four inches cube, pinned to the falling poſt, a little below the catch, to ſtop the gate from ſwinging beyond the poſt: another ſtop near the ground may be uſeful.

When gates are hung to open one way only, their heels and heads generally reſt againſt the hanging and falling poſts; but when they are hung according to this deſign, gates may be made about one foot ſhorter for the ſame opening, and conſequently they muſt be lighter, ſtronger, and leſs expenſive.

Of the hanging of Gates.

Hanging of gates.

When the two hooks in the hanging-poſt are placed in the ſame perpendicular line, a gate, like a door, will reſt in any direction in which it may be placed. But, in order that a gate may ſhut itſelf when thrown open, the hooks are not placed exactly perpendicular; the upper hook declining a little towards the falling-poſt, or a few feet beyond it. In whatever direction that hook declines the fartheſt, in the ſame direction will the gate reſt, if unobſtructed, and its head cannot then ſink any lower. Make the head deſcribe half a circle, and it will thus have attained its utmoſt elevation, and will be equally inclined to deſcend either to the right or to the left *.

Particular deſcription of the method of hanging a gate.

The following method of fixing the hooks and thimbles, will, I think, be found to anſwer very well for a gate that is intended to open only one way. Suppoſing the face of the hanging-poſt to be ſet perpendicular, and the upper hook driven in near its inner angle, as is repreſented in the preceding deſign, and that the lower hook muſt be four feet and a half below it; ſuſpend a plumb-line from the upper hook, and at four feet and a half mark the poſt; then at one inch and a half farther from the gateway than this mark, drive in the lower hook; this hook muſt project about half an inch farther from the face of the poſt than the upper hook. In the ſection or ground-plan of the gate,

* See Chap. II. of Mr. Parker's Eſſay on the Hanging of Gates; and alſo the Agricultural Report for Northumberland, by Meſſrs. Bailey and Culley.

gate, the two white circles near the hanging-post represent the places of the two hooks when brought to the same horizontal line; that nearest the gateway represents the place of the upper hook. A line drawn through the middle of these two circles, and extended each way, will, on one hand, represent the gate's natural line of rest, and, on the other, the line of its highest elevation. A gate thus hung will, when thrown open nearly to the line of its highest elevation, return to the falling-post with a velocity sufficient to resist a moderately strong wind. This velocity will be either increased or diminished, accordingly as the upper hook declines more or less from a position perpendicular to the lower hook. In order to adapt the thimbles to these hooks;—as the lower hook is one inch and a half farther from the gateway than the upper hook, the lower thimble must have its eye an inch and a half farther from the heel of the gate than the eye of the upper thimble, in order that the bars of the gate may be in a horizontal position when it is shut. And, as the upper hook projects half an inch less from the hanging-post than the lower hook, the upper thimble should be fixed half an inch nearer the farther side of the heel of the gate than the lower thimble, in order that the gate may be in a perpendicular position when shut. If the thimbles have straps embracing the heel of the gate, and proceeding a few inches along each side of the bottom and top bars, and if they are fixed to the heel bars and braces, by means of iron pins and screw nuts, great firmness will be given to the gate at those two points, which are those that suffer the greatest strains.

Particular description of the method of hanging a gate.

*** To this communication are annexed a certificate from Mr. Edward Simpson of Wooden Croft Lodge, near Barnard Castle, in favour of the advantage of these gates in saving and durability; and also a letter from Mr. T. N. Parker, author of a well-known Treatise on the hanging of Gates, expressing his approbation of the same.

Description

VII.

Description of an improved Gun-Lock, by Mr. GEORGE DODD.*

Description of an improved gun lock, by reference to the drawings.

THE figures in *Plate* I. represent Mr. Dodd's improved gun-lock with its parts in their several situations and positions. The shaded drawing, *Fig.* 1. represents the exterior parts of the lock; and *Fig.* 2. represents its interior. *Fig.* 6. exhibits, in perspective, the tumbler, the sear and the sear spring in the position of whole cock. The outline plans 3, 4 and 5 shew the several positions of the parts, at full cock, half cock, and immediately after the discharge. The tumbler A terminates on the lower side in a tail, as usual; upon which the main spring acts: but, on the upper or opposite side, it is formed so as to have two notches or bents, one very deep for the half cock, and the other shallower for the whole cock, as is seen in the figures. The circle *a*, *Fig.* 3, supposed to be described by the extremity of the bearing face of the tumbler at whole cock, is larger than that through which the extremity of the bearing face of half cock passes, see *Fig.* 5. and the center of the sear B is placed in the outer of these two circles, having the under side of its nose fashioned in the arc of the circle *b b*, described by the motion of its extremity. The bearing surfaces of the bents or notches of half and of whole cock are made to fit this face; or, in other words, they form parts of the same circle, when respectively at whole or half cock. D is the trigger, so formed and placed that, at whole cock it trips or draws out the sear, with great facility and quickness, by the action of an inner slope or face lying in the direction of a radius of the circle it describes; (see *Fig.* 4.) But when at half cock, *Fig.* 3, its action, by means of an outer slope or surface (which lies intermediate between radii drawn from the centers of the sear and of the trigger to the inner point of their contact) is so far from discharging the motion, that it tends to keep the sear more strongly in its place. These actions and properties are sufficiently evident from the figures.

Enumeration of the good qualities of this lock.

The advantages of this lock are, 1. It is fully as simple in its construction, or rather more so, than the common lock, and is therefore no less cheap and easy to be cleaned by a common soldier

* From the Transactions of the Society of Arts for 1804.

soldier or workman: 2. It is discharged very speedily, and cannot possibly catch or hang at the half cock in the act of discharging: 3. The bearing parts at half cock are extremely strong and cannot miss their hold or be thrown out of taking by any accident. In particular the trigger cannot be made to remove the sear; because its action at half cock is in the contrary direction. Hence it is much more simple and its means of safety are nearly as secure as any bolt, that is to say it is perfectly effectual as far as regards the trigger; though it does not, like some of the bolt stops, prevent the full cock being made. But on the other hand, as the inventor remarks, its security in no respect diminishes its ready use. For bolts, exclusive of the additional expence, have the disadvantage of requiring a previous operation before the piece is fit for service. Few people when alarmed have the presence of mind to unbolt, but they instantly attempt to cock. Disappointment tends to produce that agitation and confusion of mind which, at such a juncture, may occasion the loss of their lives from opponents who are little disposed to shew mercy to an enemy from whom they had no reason to expect any.

Certificates of the utility and novelty of this invention, from respectable makers, with letters of approbation from the Board of Ordnance, were exhibited to the Society of Arts; and it is probable that this apparatus will obtain the encouragement it appears to deserve. The Society expressed their sentiments by awarding the silver medal with the sum of ten guineas to the inventor.

VIII.

Investigation of the Properties of the Lines drawn in a Circle by Mr. BOSWELL *in the Tenth Volume of this Journal. By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR, *Middleshaw, April* 17, 1805.

THE theorems respecting the circle, given in your number for March last by the ingenious Mr. Boswell, will undoubtedly prove useful to the artist and practical philosopher. On this account

Propositions respecting the circle's area, &c.

Propositions respecting the circle's area, &c.

account they ought to be made as correct as possible; which has not been as yet done, either by Mr. Boswell, or by his more scientific commentator in your number for the present month. This declaration in a manner compels me to undertake the following investigation of the subject, in which I shall refer to *Fig.* 4. *Plate* XI. of your Journal for April, requesting the reader to place the letter T at the upper extremity of the vertical diameter, and C at the opposite end.

Theorem 1st. Let the circle I T F C have unity for its diameter; draw the diameters I F, T C at right angles to each other; bisect the radius T O in W; join I W, and produce it until it meets the circle again in B: these things being done, the square upon I B is equal to .8000; which exceeds .7854, or the common expression for the area I T F C by the fraction .0146.

Demonstration. Put the radius $IO = R = .5$; then $OW = \frac{1}{2}R$; since the triangle I O W is right angled at O, by hypothesis, $IW^2 = R^2 + \frac{1}{4}R^2 = \frac{5R^2}{4}$, *Euc.* 47.1. Now the triangles O I W, B I F are equiangular; because the angle at I is common to both; and the angle F B I is equal to W O I, being right, *Euc.* 31. III; consequently, as $WI : IO :: IF (= 2IO) : IB$; hence as $WI^2 \left(= \frac{5R^2}{4}\right) : IO^2 (= R^2) :: IF^2 (= 4R^2) : IB^2 \left(= \frac{16R^2}{5}\right)$; but $R^2 = .25$; therefore $IB^2 = \frac{16 \times .25}{5} = 16 \times .05 = .8000$. But $.8000 - .7854 = .0146$. Q. E. D.

Lemma. The area of the circle I T F C is equal to the rectangle under the radius O I, and the semi-circumference I T F; for this area is equal to a triangle having I O for its altitude, and the whole circumference I T F C for its base. (*Archimides de Circulo Prop.* 1st.)

Theorem 2nd. If the right line I B be the side of a square, which is equal to the area I T F C; and B G be drawn perpendicular to the diameter I F; the segment I G of the diameter I F, cut off by B G, is equal to $\frac{1}{4}$ of the circumference I T F C or the arc T B F.

Demonstration. The triangle I B F is right angled at B, 31. *E.* III. and B G is perpendicular to I F by hypothesis therefore

therefore the rectangle F I, I G = I B^2, 8. *E.* VI.; but I B^2 = the rectangle under I O and the arc I F F by lemma; consequently as F I (= 2 I O) : I O : : arc I T F: right line I G; 14 *E.* VI. hence I G = $\frac{1}{2}$ the arc I T F, = the arc T B F, Q. E. D. Propositions respecting the circle's area, &c.

Corollary 1. If I G, a segment of I F be equal to the arc T B F; draw G B perpendicular to I F; and let it meet the circle in B; the line I B is the side of a square; which is equal to the area I T F C. This is the converse of the Theorem.

Cor. 2. If any angle at the centre of the circle as I O B, be given in parts of the right angle T O F; and I G be equal to the arc T B F; find a right line N; which shall be a fourth proportional to the angles T O F, I O B and the right line I G; this line N is equal to the arc I T B; and a mean proportional betwixt I O, and $\frac{N}{2}$, is the side of a square which is equal to the area I O B I; join I B, and from the last mentioned square take the trinangle I B O; the remaining magnitude is equal to the circular segment I T B.

Cor. 3. The square upon B F is equal to the difference of the areas of the circle and its circumscribing square.

Problem. If the circumference of a circle, whose diameter is unity be denoted by 3.1416; it is required to find a right line which shall approximate very nearly to $\frac{1}{4}$ of this number, or .7854.

Construction. Draw I B, as in Theorem 1st, and make B G perpendicular to I F; then I G × 1 = I B^2, = .8000; consequently I G = .8000, which is greater than .7854. Let the reader take *g* in I G, so that I *g* may be of the required length; then as 8000 : 7854 : : I G : I *g*; but 8000 is to 7854 nearly as 55 to 54; therefore divide I G into 55 parts and I *g* will be 54 of these parts. Draw *g b* perpendicular to I G; join I *b*; and the square upon I *b* will be nearer the truth than that upon I B. If a more complete approximation be required, it may be discovered by the method given in the ninth Problem of Emerson's Arithmetic. Q. E. F.

It is the business of the practical geometrician to determine the value of these propositions in practical geometry. The ingenious Mr. Boswell considers the first theorem to be of utility; for which reason I imagine any improvement in the discovery

discovery will prove acceptable not only to the inventor, but to several of your readers who are artists and mechanical geometricians.

JOHN GOUGH.

IX.

On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the East Indies. By Dr. WILLIAM ROXBURGH, *of Calcutta* *.

DEAR SIR,

YOUR letter of the 16th of May, 1799, I received on my return to Bengal in October last; but that from the Society established in London, for the Encouragement of Arts, &c. of which you are a member, is not to be found.

Ill advised experiments for the culture of hemp in India.

I was rather surprised, on my return to Bengal, to find the directors had sent out a person (Mr. Sinclair) to establish the cultivation of hemp, a thing I had begun some time before. Even on the coast of Coromandel, ten or twelve years ago, I made a most successful trial, the result of which was laid before that government, to be sent to the Honourable Court of Directors; and again in Bengal, since my appointment to the station I now hold. Mr. Sinclair is dead, and the experiment is still carried on in a most expensive manner; whereas it could be continued where it was first begun, in the botanic garden, at no expence, and with more prospect of success. Should government continue to be the cultivator, the price will be enormous. Eighty pounds weight is all, I believe, that is yet forthcoming, and costs from 10,000 to 20,000 rupees. Such experiments throw a complete check in the way of all attempts to introduce new, or improve old, branches of agriculture and commerce. A small premium should be offered to the natives, and honorary rewards to Europeans, after the example is set on a small and not expensive scale.

Best method of promoting its cultivation.

Botanic garden at Calcutta.

The botanic garden was at first made very large, four times more than was necessary for such a garden, the intention of which

* Communicated to the Society of Arts, 1804.

which was merely to make experiments, and to invite the natives to see and profit by the examples in husbandry carried on there.

A quantity of my bow-string flax was, I understand, sent from the coast about two years ago, for the directors. I could wish to know what was done with it; for, to me, it seems to be the strongest vegetable fibre we are acquainted with. I mean to send some by the January ships from hence, through the medium of this government, and wish it may fall into your hands, and that its qualities may be properly examined by the Society for the Encouragement of Arts. I am really sorry that the letter, inviting me to become an honorary corresponding member of that Society, should have been lost. I beg you will assure the Society, that I am sensible of the honour they have done me, and shall be very happy to have it in my power to contribute my mite to promote the views of that laudable institution.

Bow string flax: the strongest of vegetable fibres.

When any new object, promising to become useful in the arts or manufactures of our country is discovered, and reported to your Society (for example, the bow-string flax,) the Society will probably address the Court of Directors, and recommend the cultivation and importation into England of the commodity itself.

Another object, of more national importance, which I recommended to this government, before I went to the Cape, was the growth of one of the most noble of the palms, the *arrow*, mentioned in Marsden's History of Sumatra, page 77, and said to yield at an early age (from five to seven years,) fibres ready prepared by nature, flexible, strong, and most durable, and the most convenient for cables and cordage of all kinds, that can be desired. It also yields great abundance of palm wine, which can be converted into sugar or ardent spirits; and when the tree is old, its pith is the basis of the sago we so much value. I have distributed many hundred plants, and have still a great number in the garden *, beside many thousand seeds in the ground. Drawings, and a description of the most valuable tree, were sent to the directors, under the name of *saguerus rumphii*; but as the trees from which they were taken have advanced in size and age, a new

Excellent qualities of the *arrow* palm; for its fibres; its wine; and its sago.

* Feb. 1801. About 100,000 plants have been reared in this botanic garden since the date of this letter.

 set

set of drawings, and a new description, of the old one corrected, becomes necessary. They will be sent to the Directors this season. The object may be such as your Society wish to attend to; and on that account I have ventured to trouble you with the above statement.

I am, dear Sir,

Your most obedient Servant,

W. Roxburgh.

Calcutta, Dec. 24, 1799.
Robert Willet, Esq.

Dear Sir,

Experiments on hemp.

MY letter of the 24th of December, 1799, I am afraid, has not reached you. The experiments on hemp, therein mentioned, have not, I believe, thrown much additional light on the subject. My friend, Capt. Burrows, of the Earl Howe, has done more to make our own indigenious species (the *sun* of the Bengalese) better known, than any other person I am acquainted with.

Comparative examination of vegetable fibres.

For these last twelve months my attention has been much taken up in collecting and comparing the various vegetable fibres of Asia, &c. used for cordage, cloths, and paper. The result of these I have lately presented to the supreme government, to be sent to the Hon. the Court of Directors, in reply to the 79th paragraph of their general letter of the 7th of May last. This paper may be interesting to your Society, particularly at this time, when the attention of all good patriots is drawn towards the discovery of a substitute for Russian hemp. This paper, with my former essays, contain much information on the subjects therein mentioned.

Necessity of a substitute for Russian hemp.

The *sun* plant is most promising.

The *sun* of the Hindoos, which is the prepared fibres of the bark of a well-known Indian plant, the *crotalaria juncea* of Linnæus, still appears to me to be the most promising substitute for hemp which has come to our knowledge; I mean, when every circumstance, relative to its quickness of growth, its being already universally known and cultivated by all the nations of India, its low price, pliableness, strength, durability, &c. &c. are taken into consideration. All that can be necessary for the procuring and transporting to England any quantity of this material, is to ensure the cultivator a certain

certain price, and ready market for the commodity; and to have it properly cleaned and packed, to render the freight as low and convenient as poſſible. Cleaned ſamples of this very ſubſtance were ſent by me, ſix or ſeven years ago, to the Directors; ſo that the fault is not mine, if it is not already better known than it ſeems to be.

The diſcovery of a ſubſtitute for Ruſſian hemp is certainly an object of the firſt magnitude. If *ſun* is found to be the beſt ſubſtitute yet diſcovered, and coſts in India, ſay, when properly cleaned, ten pounds per ton, and the freight ſixteen pounds, there will ſtill be a conſiderable profit to the merchant, particularly in times of war; for, I believe, it rarely happens that hemp ſells ſo low in London as thirty pounds per ton. Should the ſubject appear of conſequence to you, I beg you will call the attention of the members of your Society to it. It can be afforded at a reaſonable rate.

That I may not encroach too much on your time, I will cloſe this letter by referring you to my friend Mr. Boſwell, the late Marine Storekeeper and Naval Paymaſter, for any farther information you may want. I ſend this package by him. Mr. Babb, late a Member of the Board of Trade here, can alſo give you much information.

I am, dear Sir,
Your moſt obedient Servant,
W. ROXBURGH.

Calcutta, Feb. 27, 1801.
Robert Wiſſet Eſq.

Obſervations on the Culture, Properties, and comparative Strength of Hemp, Sun, Jute, and other Vegetable Fibres, the Growth of India, communicated by Dr. ROXBURGH.

HEMP, conſidered merely as an article of trade, is an object of the firſt importance to the merchant; but, when we reflect on its various uſes, and obſerve that hardly any art can be carried on without its aſſiſtance, or of ſome other ſubſtitute; the objects it embraces are immenſe, and there are few that better deſerve the attention of the philoſopher or intelligent artiſt. Cordage makes the very ſinews and muſcles of a ſhip, and every improvement which can be made in its preparation, either in reſpect to ſtrength, pliableneſs, or Importance of hemp and its very extenſive uſe in ſociety.

or durability, or in bringing to light substitutes equally good or better, particularly where hemp itself cannot well be had, must be of immense service, particularly to the mariner, and to the commerce and the defence of nations.

Its cultivation in our colonies intitled to support and encouragement.

The cultivation of this important plant in our colonies has not only, at all times, met with encouragement from the government, but also of late from the East-India Company in Bengal, where extensive experiments were begun by the late Mr. Sinclair, and after his death carried on by Thomas Douglas, on the culture of hemp and flax, on account of the Honourable Company. A clear and impartial statement of these trials is to be wished for, as it will, no doubt, throw much light on the cultivation of hemp in India, and enable us to proceed with greater prospects of success than ever.

It may be very beneficially produced in India.

My own experiments and inquiries on the same subject, both on the coast of Coromandel and Bengal, have been many, though not extensive. Their result leads me to think, that hemp may be cultivated to great advantage over the interior parts of Bengal and Behar, where the seed should be sown about the beginning of the periodical rains, or earlier, if there have been frequent showers, on elevated spots of rich loamy soil, such as the Ryots cultivate tobacco, sun, and paat on, near their habitations. In situations of this nature it thrives well, and will be easily attended to. At first, some encouragement will certainly be necessary, to induce the Ryots to undertake this new branch of agriculture. For, although the plant is perfectly familiar to every Hindoo, yet the cultivation on an extensive scale, for the fibres of its bark, is perfectly unknown to them. I would therefore suggest, that they should pay no rent for the ground so occupied for a certain period; that seed should be given gratis; that they should be ensured a certain price for the hemp; and finally, a reward or premium to the person or persons who produced the greatest quantity of the best hemp within a stated period.

The Hindoos cultivate it only for the seed.

Season for growing it.

In many parts of Bengal, particularly where the land is so low as to remain humid through the dry-weather season, hemp thrives luxuriantly during the cold season; but the water is then too cold for macerating the plants to the greatest advantage: one day in June, July, or August, has more effect in loosening the bark, than eight in December, January, or February; consequently, the prolonged immersion injures the quality

quality of the hemp much. The rainy ſeaſon is therefore preferable for the cultivation and maceration, even if the plant grew better during the cold, which is by no means the caſe, particularly on lands elevated above the level of the annual inundation of the low rice-fields. We muſt therefore content ourſelves with one crop in the year; for it is a very falſe notion, and a very prevailing one, that the fertile fields of Aſia produce at leaſt two crops annually; as well might we ſay, that the fertile lands of England yield at leaſt two, becauſe a well-managed garden, near London, or ſome other large city, will produce repeated crops in the year: ſo in India, by great care and induſtry, a ſpot here and there will produce two or more crops. The burning heats of Aſia, while they laſt, are as unfavourable for vegetation, as the froſts of winter in Europe. One annual crop only can be had.

Beſides hemp and flax, the vegetable kingdom, particularly that natural diviſion called by Linnæus, Columniferæ, abounds in plants which produce materials fit for cloths, paper, and cordage: almoſt every nation or country poſſeſſes ſomething of the kind peculiar to itſelf. To aſcertain what theſe are, as well as to find out new ones, to try their comparative ſtrength, durability, texture, &c. has, at various leiſure hours, employed my attention for many years paſt. Drawings and deſcriptions of many of them have been already laid before the honourable Court of Directors. There are, however, ſome others which remain to be brought under view and compared with the kinds we are beſt acquainted with, which I have attempted in the following experiments. Many other ſorts are alſo mentioned by various authors and travellers, of which I know nothing more than the names. Two of theſe are mentioned by Marſden, at pages 75 and 76 of his hiſtory of Sumatra. Others are peculiar to Pegu, &c. &c. Other vegetables particularly the columniferæ, fit for the uſes of hemp.

Theſe I muſt omit for the preſent, and confine myſelf to ſuch as I am more intimately acquainted with. The better to enable me to proceed in this inquiry, I have cultivated, in the Botanic Garden at Calcutta, many of the plants themſelves which produce the materials hereafter mentioned; prepared their fibres in general by maceration, &c. as with hemp and flax in Europe. And, to compare their ſtrength (plain, tanned, and tarred,) had then made into cords, compoſed of three ſimple yarns, as nearly of the ſame ſize and hardneſs as a Deſcriptive outline of the author's experiments on various plants for cordage, &c. Preparation of the fibres.

Cordage manufactured, and tried in the white, and also tanned and tarred.

Hindoo rope-maker could make them; but, in ſpite of my utmoſt care, they were always too hard twiſted to be of the greateſt poſſible ſtrength *. Of each ſort there were ſix, when there was a ſufficient quantity of the fibres. Three of them were about the ſize of a log-line, and three a ſize larger than a whip-cord; one of each ſize and ſort was kept white: the average number of pounds which broke them, (for repeated trials were made, and always with lengths of exactly four feet), will be found in the firſt and ſecond columns on the right. One of each was tanned † with the aſtringent fruit called *gaub* by the Bengaleſe, (Embryopteres Glutinifera, Roxb. Coromandel Plants, Vol. I. No. 70.) Their ſtrength is repreſented in the third and fourth columns of the following table. And, laſtly, one of each ſort was tarred: their reſpective ſtrengths will be found in the fifth and ſixth columns.

Tanning ſtrengthens animal fibres, but quere vegetable?

We know the tanning principle ſtrengthens the fibres of leather, (animal fibres) but are not ſo clear that its operation on vegetable matter is uniformly the ſame. The attention beſtowed to aſcertain this point in theſe experiments will, at leaſt throw ſome light on the ſubject; and may induce others, better qualified, to extend the inquiry (here in India), where tanning materials abound.

* The experiments of Reaumur, Sir Charles Knowles, and Du Hamel, uniformly prove, that when hemp-rope is twiſted to the uſual hardneſs, which is that which brings them to two-thirds of the length of their reſpective yarns, their ſtrength is leſſened by nearly one-fourth, when compared with ropes conſiſting of the ſame number of the ſame yarns twiſted up to only three-fourths of their length. There will be no difficulty in accounting for this difference, if we conſider that a ſkein of fibres may be twiſted ſo very hard, as to break with any attempt to twiſt it harder. In this ſtate the fibres are already ſtrained to the utmoſt, and cannot ſupport any weight or additional ſtrain.

† The idea of tanning cordage is far from being new; for the fiſhermen of Aſia, as well as of Europe, not only tan their nets and lines, but alſo their ſails, to give them additional ſtrength and durability. The ſame proceſs might be productive of the ſame effects, if employed on cordage made of the materials (No. 2, 5, 6, 7, 8, and 15), ſpecified in theſe experiments, which induced me to recommend its being tried with ſun cordage, in my letter to the Board of Trade, in Auguſt, 1797.

Another

Another point of the utmost importance to be ascertained is whether tanned ropes will be preserved by the tan, with which they are impregnated, when stowed away wet; as tarred ropes are preserved by the tar when so circumstanced. It is nevertheless a well-known fact, that tarred cordage, when new, is weaker than white; and that the difference increases by keeping. Tar can therefore only be employed to preserve cordage, and not to strengthen it; so that if tan will add strength, or even not weaken vegetable fibres, and at the same time preserve them; of how great advantage to the nation would the discovery of a convenient practicable process be. For all cordage, exposed to be alternately very wet or dry, requires to be impregnated with a preservative. And, to conclude this long note, I beg leave to observe, that tar is not the produce of the warmer parts of Asia. Are we, therefore, to conclude, that no material, the produce of these parts, can be applied for the same end? Let us not entertain any such idea. Nature is abundantly kind, and furnishes every country and climate with what is most proper for the use of its inhabitants.

Whether tan will preserve wet rope?

Tar does not strengthen cordage; but the contrary.

Tar not produced in tropica Asia.

The annexed statement of the experiments made on the substances there specified, can only be deemed an attempt towards ascertaining their relative strength; and though they are the average result of several trials made on the strength of each cord, plain, tanned, and tarred, yet I must acknowledge they cannot be deemed any thing more than a first essay, chiefly owing to the lines being in general ill laid, some more and some less twisted, and by no means to be compared with those of Europe. For in some instances, I found a small one sustain a greater weight than a much larger, made exactly of the same materials. It is therefore my intention to repeat them on a larger scale, and, if possible, with better made lines; for every thing depends on their being exactly of the same size and degree of twist.

Statement of experiments on the cordage.

COMPARATIVE

COMPARATIVE STATEMENT OF THE EXPERIMENTS.

Names of the Plants, and brief Remarks on the various Materials employed in these Experiments.	Average weight at which each sort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
No. 1. English Hemp: a piece of new tiller-rope, opened out and made into cords, like the rest,	105	65	..	..	..	..
2. Hemp (Cannabis), the growth of this season, from the Company's Hemp Farm, near Calcutta,	74	50	139	60	45	46
3. Coir: the fibres of the husk of the Cocoa-nut; much used for cables and cordage over Asia,	87	60	..	..	..	.
4. Ejoo (Saguerus Rumphii): the black horse-hair-like fibres, which grow round the trunk of this species of Sago Palm,	96	79	..	..	..	..
5. Robinia Cannabina, Dansha of the Bengalese: the fibres of its bark, prepared by maceration from the plants that had nearly ripened their seed, they are then of a dusky grey colour, and harsh nature,	88	64	101	55	84	39
6. The fibres of the bark of No. 5, prepared by maceration, from plants coming into blossom; at which time they are beautifully white, soft, and glossy,	46	20	61	35	48	30
* 7. Crotolaria Juncea, Sun of the Bengalese: the fibres of its bark, and universally known over India,	68	47	69	55	60	37
8. Corchorus Olitorius, Bunghi-paat of the Bengalese: the fibres of its bark, called Jute by the same people,	68	47	69	59	61	36
9. Corchorus Capsularis, Ghee-nalta-paat of the Bengalese: the fibres of its bark they call Nalta Jute,	67	47	..	..	..	..
10. Flax (Linum Usitatissimum): the growth of the Company's Hemp Farm, near Calcutta	39	37	..	..	..	..

* A cord, a very little thicker than a log-line, made of sun sail twine, broke with 148 pounds when dry; but, on being soaked in cold water for 24 hours, it bore, while wet, 222 pounds. This difference requires to be farther inquired into.

COMPARATIVE STATEMENT OF THE EXPERIMENTS—*continued.*

NAMES of the PLANTS, and brief REMARKS on the various MATERIALS employed in these EXPERIMENTS.	Average weight at which each sort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
11. Agava Americana: the fibres of its leaves. They are of a coarse harsh nature, and white, - - - - - - - - - - - - - - -	110	71	79	78	78	38
12. Aletres Nervosus—In Sanscrit, Murva; Murga of the Bengalese: the fibres of its leaves made into these cords, after having been kept above one year, -	120	52	73	42	48	43
13. Theobroma Augusta, Linn.—Abroma Augusta, Hort. Kew.—Abroma Wheelerii, Kœn.—Woollet-comal of the Bengalese: the fibres of its bark prepared by maceration, &c. like hemp, - - - - - - - - - -	74	61	58	51	44	29
14. Theobrama Guazuma, Bastard Cedar: the fibres of the bark of some straight, luxuriant, young plants, - - - - - - - - - - -	52	48	..	47	45	30
15. Hibiscus Tiliaceus, Bola of the Bengalese: the fibres of its bark, and employed for cordage by the inhabitants of the South-Sea Islands, &c. - - - -	41	..	62	39	61	..
16. Hibiscus Manihot, a tall white-flowered variety: the fibres of its bark, which are beautifully white, glossy, and soft, - - - - - - - - -	61	..	..	..	..	..
17. Hibiscus Mutabilis: the fibres of its bark, and they are of a harsh nature and ill coloured, - - - - - - - - - - - - - - -	..	45	53	46	..	..
18. Hibiscus, a new species, from the Cape of Good Hope, said to be a native of Caffraria, where the fibres of its bark are spun, - - - - - - -	..	22	..	..	..	..
19. Bauhinia, a large scandent species: the fibres of its bark, cleaned without maceration, and used to make ropes, &c. of by the people of Napaul, where the plant is common, - - - - - - - - - - - - -	69	39	..	..	..	..
20. The same as No. 19, only maceration was used to help to take the bark off the twigs with more care, - - - - - - - - - - - - -	56	41	..	..	..	..
21. Sterculia Villosa: the fibres of its bark. Cords are made of them by the natives of the eastern frontier of Bengal, to bind wild Elephants, when first taken,	53	..	..	..	..	..

The

The cords, when the trials were made, had been kept about six months after they were prepared, chiefly with the view of allowing the effects of the tan and tar to take place. The result of these experiments show, that tan has in general added strength, while tar has had a contrary effect; and in no instance is this more clearly evinced, that in the common hemp (Cannabis) cultivated in Bengal.

Ejoo and Coir.

To Ejoo and Coir, neither tan nor tar seem applicable; and in several of the other experiments, I had not a sufficient quantity of the materials to make the necessary number of cords, viz. six of each sort, to try with tan and tar, as well as in their natural state. At some future period, I hope to be more fortunate in procuring larger supplies of the materials, and also to add some other other sorts, such as the—

Bow-string fibre.

Rajemahl bow-string fibre, the produce of a new species of Asclepias, discovered by William Roxburgh, junior, amongst the Rajemahl Hills;

New Zealand hemp.

New Zealand Hemp;

Hisbiscus Cannabinus, and some other of the same natural order (Columniferæ); (for in general their barks abound with strong fibres; witness the foregoing table, where six of them are to be found);

The leaves of a new species of Andropogon, &c. &c.

APPENDIX,

Containing Remarks on some of the Plants mentioned in the foregoing Table.

Remarks on hemp.

No. 2. Hemp, or Cannabis Sativa.—Banga, in Sanscrit; Bunga, Bungh, or Bung, of the Hindoos; Bang, of the Persians; Kinnub, of the Arabians, is no doubt our own famous plant, now so common and useful in Europe. I have at different times examined various figures and descriptions, as well as the plants reared from Europe seed, comparing them with our Indian plant through its various stages, and can discover no difference whatever, not even to found a variety on. Perhaps few vegetables, so widely diffused over almost every part of the known world, and under the immediate management of man, have undergone less change. It is perfectly familiar to all the nations of India, I may say of all the warmer parts

It is well known in India; but not for its fibres;

parts of Afia; yet I cannot difcover that the fibres of the bark have ever been employed for any purpofe. It is cultivated in fmall quantities every where, on account of its narcotic qualities.—The leaves of the male plant, and flowers of the female, are the parts in moft general ufe.

I have repeatedly applied for the feeds of all plants reared in China, and other countries to the Eaftward of the Bay of Bengal, as well as to almoft every other part of India we have any communication with, for an account of the plants employed to fupply materials for clothes and cordage, and for their feeds; but could never learn that Cannabis was one of them; nor were its feeds ever fent to me as fuch. nor in China, &c.

No. 4. The great ftrength of this fubftance makes it a very defirable object. For a defcription and drawing of the tree I refer to thofe * which accompanied my letter to the Moft Noble the Governor-General in Council, under date the 2d of January, 1800. The fibres employed in thefe experiments were taken from trees growing in the Botanic Garden at Calcutta, where they thrive well. I could obferve, during the trials made in breaking the cords of this fubftance, that they were not fo elaftic as thofe of Coir, which will probably render it lefs fit for cables, but better for many other ufes. Coir is certainly the very beft material yet known for cables, on account of its great *elafticity* and ftrength. Ejoo; very ftrong, but not elaftic.

Nos. 5, 6, 7, 8, and 9. Thefe four plants have already been figured and defcribed by me, in a memoir fent through the Governor-General in Council, to the Hon. the Court of Directors, in December, 1795. Since writing that paper, I have learned, that *fun* (Crotolaria Juncea) is almoft univerfally employed, over the warmer parts of Afia, for cordage. On the Malabar coaft, I find it is generally named by the gentlemen at Bombay after the province where reared. It is ufed in that place for lacing their cotton bales, on account of its great ftrength. Samples of three forts, viz. Malwan, Rajapore, and Salfette, were fent to me, from them, by Dr. William Hunter; and am induced to think, little or no maceration is Other plants: The *fun* or crotolaria juncea.

* A former fet, No. 1179, fent in to Government on the 23d of November, 1797, were not fo correct as could be wifhed, on account of their having been taken from young trees, juft coming into bloffom the firft time.

employed

employed in taking the bark from the ftalks, or in cleaning the fibres, which may add to its ftrength: for certainly maceration, particularly if long continued, muft weaken frefh vegetable fibres confiderably. The fame gentleman fent me feeds of the Salfette fort: they have produced plants now in bloffom, and from them have afcertained the identity of this fpecies.

In fome parts of Bengal, a moft luxuriant variety is cultivated immediately after the rains, which often grows to the height of twelve or fourteen feet; while the common fort is generally reared in Bengal during the early part of the wet feafon, and grows to only about half the height of the former.

Danfha of the Bengalefe;

I muft farther obferve, that the fibres of No. 5, poffefs great ftrength, and it feems to me to be one of the moft fit of any of our Indian productions for cables and cordage. The plant grows generally to the height of from fix to ten feet, the fibres long, but harfher than thofe of hemp, if not cut at an early period. It is very generally cultivated about Calcutta during the rains. An acre yields of the half-cleaned fubftance (the ftate in which the natives carry it to market), about 600 lbs. weight, and fells for about a rupee and a quarter per maund of 80 lbs.

beautiful but weak fibres.

No. 6. By cutting No. 5, the laft-mentioned plant, when beginning to bloffom, we have the moft beautiful fhining white fibres that can well be conceived, but (by my experiments) greatly weaker that when the feed is fuffered to be nearly ripe before the plants are cut.

Flax cultivated in India for its feeds only.

No. 10. Flax, the plant, is very generally cultivated during the cold feafon, over the interior parts of Bengal and Behar, merely for the feeds, from which oil is obtained. The flax itfelf the Hindoos fet no value on; for, after they have gathered the feed, they throw away the ftalks as ufelefs, having no knowledge of the fibres which their bark yields. Samples of the flax have repeatedly been procured by the Board of Trade, and fent to England to the Hon. Court of Directors; fo that it is from England we may expect to learn its properties. If the flax has been found good, large quantities may be reared at a fmall expenfe; as the feed alone, which the crop yields, muft be more than equal to the charges, to render it profitable to the farmer.

No.

No. 11. This Agava is of ſlow growth; on that account I doubt if ever it can be advantageouſly cultivated; but, where found wild in plenty, it may be manufactured at a trifling expenſe. Its great ſtrength renders it an object worthy of attention. The fibres are coarſe, conſequently rope made of them harſh to the feel. Agava Americana; of no great promiſe.

No. 12. Drawings, and a deſcription of the plant Aletris Nervoſus Roxb. the method of extracting the fibres, with a quantity of the ſubſtance itſelf, were ſent to the Hon. the Court of Directors, through the Madras Government, above ten years paſt. I alſo gave a large quantity to Mr. Bebb, when he left Bengal in January, 1800, to take to England with him for trial there. The plant grows fully as well, and is as common as on the Coromandel coaſt. There has lately been about a biggah (third of an acre) planted out with it in the Botanic garden, the better to determine the expenſe, and the annual produce of any given quantity of ground. Aletris nervoſus.

No. 13. This plant, a native of various parts of India, New South Wales, Philippine Iſlands, &c. has been long known to botaniſts; yet I cannot, with all the attention that I have been able to beſtow on the ſubject, find that the fibres, ſo abundantly interwoven through its bark, have ever been uſed or even taken notice of by any other perſon; ſo that I think we may look upon it as a new diſcovery, deſerving of more than common attention, on account of the beauty, fineneſs, and ſtrength of theſe fibres. Abroma auguſta, very promiſing.

It is perennial, grows luxuriantly in the Botanic garden, and has been cut down twice within theſe ſix or ſeven months; ſo that I think it will, at leaſt, annually afford two or three crops of ſhoots fit for yielding this ſubſtance. My experience does not yet enable me to ſtate how much may be the yearly produce of an acre, but can venture to prognoſticate as large a produce as can be obtained from an acre of Buncha, Jute, Sun, Hemp or Flax. Its growth.

To render this bark ſeparable from the half-ligneous ſhoots it covers, to ſoften its external lamina, or epidermes, and the parenchymatous ſubſtance which firmly connects the fibres in their natural ſtate, maceration in ſtagnant water for from four to eight days, during the warmer parts of the year, anſwers well whilſt three times as many days are ſcarce ſufficient during the cold ſeaſon; indeed, the proceſs is ſcarcely practicable then; beſides, the fibres are greatly weakened by the length of the maceration. Treatment to obtain the fibre.

Immediately

Immediately on being taken out of the water, and while wet, the shoots are singly taken in the hand, rubbed with some coarse materials, such as a little dry grass or gunny, to remove the exterior pulpy lamina or epidermis of the bark, which is destitute of fibres. This part of the process is easily effected; and when done, the clean shoots are to be made up into small bundles, and placed under weights, or some other equal pressure, to keep them firm at the middle and top, either under the surface of the waters, or out of it; the fibrous bark is then separated with the fingers from a small portion at the end of the ligneous shoot or stalk, which the operator takes hold of, and draws out one by one; when these are removed, the pulp, or parenchyma, which fills the interstices between the fibres, and connects them together, forming in the *living plant* that part of the bark which may be called its inner lamina, or cellular tissue, is immediately washed out in cold water, and the clean fibres spread out in the sun to dry. Such was the simple process by which this substance (which may very properly be called Indian Hemp or Flax) was prepared.

I have now under cultivation about the third of an acre (a biggah) of ground in the Botanic garden with this plant; the result shall be carefully noted from time to time. It might have been prudent to have withheld this account until that time; but the strong desire of making known a discovery, which may in a short time become beneficial to the public, induces me to be thus precipitate.

For a farther account of this plant, I beg leave to refer to my drawing and description thereof, sent to the Hon. the Court of Directors some years ago, and numbered 415.

W. ROXBURGH.

Botanic Garden, near Calcutta,
Jan. 31, 1801.

Additional Experiments on the Strength of Sun, (No. VII.)

Additional experiments on the strength of *sun.*

Some tanned sail-twine, made of this substance four years ago, for the inspection of the Marine Board, was made into a cord of three strands; each of the strands composed of four threads of the sail-twine.

Some more of the same sail-twine, tanned twelve months ago, was made into a similar cord, and another was made of the white unprepared twine.

The

The firft, which had been tanned four years, broke with 110 lbs. when dry, and with 130 lbs. after having been fteeped in water 24 hours.

The fecond, which had been tanned one year, broke with 123 lbs. when dry, and with 140 lbs. after fteeping 24 hours.

The third, or plain white, broke with 148 lbs. when dry, and with 222 lbs. after fteeping 24 hours.

X.

Defcription of the Bavarian Method of evaporating Saline Waters. By M. Bonnard.

THIS new method, practifed in Bavaria, has been introduced into the falt work of Moyenvie, by M. Cleifs, infpector of the falt-works of Bavaria. Bavarian method of evaporating falt waters.

The pans are compofed of fquare plates of caft iron, of 4 millimetres in thicknefs, and 4.76 centimetres on each fide. Thefe plates are joined by their edges, which are turned downwards, and confequently without the pan: they are folidly united by a piece in the form a fquare gutter which receives the edges, and is fecured by a great number of fcrews.

An evaporating houfe is compofed of fix pans, of this conftruction, difpofed in two rows; but thefe pans have different ufes, which require a particular arrangement.

That in the middle of the back row is the fmalleft; it has no particular fire-place, but it is heated by the junction of the chimnies from the other fire-places. The falt-water depofits its impurities in it; it is called the fmall pan.

From the fmall pan the falt water paffes into the graduating pan, which is lower than the firft, and placed in the middle of the front row; it is there kept in a ftate of conftant ebullition: the water is concentrated in it to 20 degrees, and depofits a part of its fulphated lime.

From the graduating pan the falt water paffes into the preparing pans, which are lower than it, and fituated at the two extremities of the back row: there it is alfo kept conftantly boiling,

Bavarian method of evaporating falt waters.

boiling, it is completely concentrated, and depofits all its fulphate of lime; it is then paffed into the cryftallizing pans, ftill lower than thofe of preparation, and placed at the two extremities of the front row: there the water fcarcely boils, and the falt cryftallizes.

Each pan, with the exception of the fmall pan, has a particular fire-place, the chimnies of which pafs round the fides of the pan: they unite under the fmall pan, by which means there is little heat loft.

Thefe pans are placed two and two in chambers of wood, the joinings of which are well fecured, which clofe them hermetically: thefe chambers are low, and their ceilings are perforated in the middle with holes terminating in a tube, by means of which the aqueous vapour is difengaged with rapidity. The chambers for the preparing and cryftallizing pans have their ceiling pyramidal, or in the form of a reverfe hopper, while that for the fmall pan and the graduating pan is horizontal.

The faline waters are paffed fucceffively into thefe four kinds of pans; the workmen penetrate into the chambers, in the midft of the vapour, to open the communications. This operation is performed every fix hours, and the water in each pan is reftored to the level at which it ftood fix hours before. Every three hours the falt in the cryftallizing pans is collected, it is brought with fcoops to elevations on the front edge of the cryftallizing pans, where it drains; it is afterwards carried into drying rooms, which furround the outfide of the chambers: thefe are fpaces covered with iron plates; they are warmed by heat-tubes leading from the fire-places.

Every eight days they take away the fulphate of lime, throw out the mother-waters, and break the fhell, that is to fay, the incruftations of falt which adhere to the bottoms of the pans: every twenty-four days the work is entirely ftopped to repair the pans; an operation which is performed by the workmen themfelves.

Economy of fuel.

It has been found that this method of evaporation affords a faving of more than one-third of the fuel.

An improvement has lately been made in this procefs at Dieufe: the fmall pan has been fuppreffed, and the drying rooms have been replaced by auxiliary pans, in which a coarfe falt is made.

The

The heated drying rooms are uselefs when the humidity of the falt arifes from the muriate of lime it contains.

Bavarian method of evaporating falt waters.

Explanation of the Plates III. and IV.

Fig. 1. Plan of the pans.
No. 1. Small pan.
No. 2. Graduating pan.
No. 3. Preparing pan.
No. 4. Cryftallizing pan.

The difpofition of the plates of iron which compofe thefe pans, is fhewn in No. 2.

a, a. Elevation on which the falt is placed to drain, as it is taken from the cryftallizing pans.

b, b, b, Wooden partitions which feparate the chambers.

c, c, c, A raifed wooden ledge which furrounds the pans.

Fig. 2. Section of the evaporating chamber which contains the pans 1 and 2.

d, d, d. Heat tubes which give heat to the fmall pan, and contribute to heat the others.

e, e, e, Fire place for the pans.

i, i, i. Pillars of caft iron under the gratings *g, g, g,* which fupport the bottom of the pans.

h. Wooden chamber which contains the two pans.

k. Opening by which the vapours efcape.

Fig. 3. Section of the evaporating chamber which contains the pans 3 and 4.

a. Elevation on which the falt from the cryftallizing pans is placed to drain.

The other letters indicate the fame parts as in the preceding figures.

Fig. 4. Method in which the plates of iron are joined to form the pans.

a. The iron plate.

b. The iron gutter which receives the edges of the plates, and is ftrongly faftened with fcrews.

i, i. Pillars of caft iron which fupport the bottom of the pan.

XI.

Letter of Inquiry respecting the late Dr. IRVINE'S *fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Answer by Mr.* IRVINE.

Request that Mr. Irvine would describe his father's experiment on the capacities of ice and water.

A CORRESPONDENT would be much indebted to Mr. Nicholson if he would convey by publication in his Journal, or otherwise, the following request to Mr. Irvine. That gentleman in his paper in the Journal of last month * mentions his having in his possession the experiments of Dr. Irvine on the capacities of bodies. It has always been a wish with these chemists who have attended particularly to this subject to know in what manner Dr. Irvine made the experiment to ascertain the comparative capacities of ice and water. If either of the bodies during the experiment change its form, if the ice were melted, or the water congealed, it becomes less decisive, as it may be objected by those who maintain the opinion that latent caloric exists in bodies either in whole or in part in a state of chemical combination, that the result might arise from such a combination, and not from a change of capacity. But if the experiment were made in such a manner that no change of form took place, which though difficult is possible, Dr. Irvine's theory, which is so much superior to the other, is unequivocally established. It would be conferring a favour on the chemical world, if Mr. Irvine would take the trouble of saying in what manner the experiment was conducted, providing such a notice would not interfere with the intention he has announced of giving a more full account of his father's investigations, an account which would be eagerly received by chemists.

Change of form would influence the result.

September 24*th*, 1803.

Reply by Mr. IRVINE.

The experiments may be made without being liable to the objections before stated;

WITH regard to the enquiry of Mr. Nicholson's correspondent, it would give me pleasure to inform him of any circumstance within the sphere of my own knowledge that should tend to add to the illustration or proof of my father's theory.

* Sept. 1803. See our Address to Correspondents of last month.

There

There does not seem to me any difficulty in explaining satisfactorily how experiments on the capacities of ice and water may be conducted without being exposed to the objections above stated; at the same time, though these experiments may be conclusive as far as they go, I have not been accustomed to consider them as altogether so decisive of all arguments upon this subject.

—but may not be unexceptionable.

In a general way then the experiments of Dr. Irvine were conducted in the following manner. The capacity of water being taken at unity, pains were used to ascertain the capacities of mercury, river sand, pounded glass, and iron filings, with respect to water, and consequently to each other in the ordinary manner. It is at present of no importance what proportional quantities of the materials were employed. This being done, the capacity of one of these substances was experimentally compared with that of pounded ice or snow.—Every precaution was used to ensure success. The weight and capacity of the vessel was determined, and the colder and hotter body alternately added to the other. The temperature of the air was always below 32, as was that of all the materials and of the vessel. No water could therefore be formed. In his first experiments I believe Dr. Irvine used mercury, but afterwards I know that he preferred iron filings and sand. For example, if the vessel and room were at 11°, let half an ounce of powdered ice from distilled water, at temperature 30° be poured on four ounces of iron filings at 11°, let the temperature of the mixture be noted after stirring, the due allowances made for the heat gained by the vessel and the air, and a proper calculation made. Let this experiment be reversed by cooling the ice to 11°, and pouring the iron filings at 30° upon it, and let a calculation from this be compared with the former and corrected by it. Finally, let these experiments be compared with others where different quantities of materials are used, and of different temperatures, and you have a view of the method employed for determining this point by Dr. Irvine, which does not appear to be objectionable upon other grounds than all experiments for ascertaining capacities are, none of which have any pretensions to perfect accuracy. Dr. Irvine was far from being satisfied that his experiments were mathematically precise. But he uniformly found the capacity of ice to be less than that of water, and that in a greater ratio than is generally allowed.

Account of Dr. Irvine's method of experimenting.

The capacities of mercury, sand, pounded glass and iron filings were taken with respect to water.

One of these was then examined against pounded ice at a temperature of the place always under 32°.

Example.

 With

The common notion, of ice and water being directly applied to each other, unfounded and impracticable.

With respect to the common notion that experiments on the capacity of ice are made by mixing it with water, it is altogether false in the case of Dr. Irvine. No doubt this may be done theoretically. It is easy to say that ice loses so many degrees which heat water only so many. But then no ice must be melted, which if not impossible is extremely difficult, or what is equally so, the quantity melted must be found, and an allowance made for the latent heat, which is itself not precisely ascertained, at least not with sufficient accuracy for this purpose. I have only to add that this gentleman's enquiries would have been earlier noticed if I had been informed of them, which I was only a few days ago.

W. IRVINE.

April, 1805.

XII.

Short Account of some of the most remarkable Facts and Observations in an Aerostatic Voyage, made from Petersburgh, by Messrs. ROBERTSON *and* SACHAROFF, *under the Sanction of the Imperial Academy.* W. N.

Aerostatic voyage from Petersburgh.

THE notice of an aerostatic voyage performed by Messrs. Robertson and Sacharoff from Petersburgh, under the direction of the Imperial Academy of Sciences, June 30, 1804, of which an abridged account was given to the National Institute of France *, is very interesting, from the scientific views and conduct of the managers Lowitz and Robertson, as well as for its other particulars.

Ascent of the balloon with two observers.

Their balloon was a sphere of 30 feet diameter, and rose at a quarter past seven in the evening, with the ascensional force of one pound, the whole weight of solid matter (including 110 pounds of sand for ballast) being 622 pounds. When they were over the river Neva, at the elevation of 108 toises or 620 feet, they descended a little by the condensation of the gas; but rose again by throwing out a little of the ballast. The usual phenomenon of a slow rotation of the balloon presented itself, which doubtless arose from the unequal action of the air against an irregular surface, as we see in most other bodies rising or falling in a fluid.

* Inserted in the Annales de Chimie, LII. 121.

But

But one of the moſt ſtriking circumſtances attending this voyage is the rational means which theſe philoſopers made uſe of to determine, and in a certain degree to regulate their courſe. They made uſe of two inſtruments, a log and a teleſcope. The log conſiſted of two ſheets of very thin paper, blacked, and fixed at right angles to each other by a very light croſs of wood. This was ſuſpended from their car by a ſtring of ſixty feet in length, and affording a different reſiſtance to the air from that of the balloon itſelf,† it was found to draw the ſtring out of the perpendicular direction, or as the narrators ſay, *to follow the balloon:* ſo that by its poſition determined by compaſs they could aſcertain what direction they were purſuing. It alſo ſhewed by its relative riſe and fall whether the apparatus was deſcending or aſcending, before their barometer had indicated the ſlighteſt change.

Their direction observed by a floating log; —of paper, &c.

Their teleſcope was applied to ſhew the direction of their courſe, and muſt have been much leſs ſubject to doubt than their log. Its application would be univerſal and perfect, if the earth could be ſeen at all times from the elevated regions of the air. It was directed perpendicularly downwards by means of a plumb-line, and having a conſiderable magnifying power, the objects upon the ſurface of the earth were ſeen moving acroſs its field of view, and their direction would moſt clearly aſcertain that of the car itſelf, and alſo its velocity. If, for example, the magnifying power were fifty times and the field of view one degree, the viſible ſpace included in that field from an elevation of two miles would be about 180 feet in diameter, in which objects of ſix or ſeven inches broad might be very well diſtinguiſhed through a favourable atmoſphere; and at ſo low a velocity as one mile an hour the whole field of view would be paſſed over in about twelve ſeconds. Hence we ſee that the method affords a conſiderable degree of accuracy, and will not in general require any great power of magnifying or delicacy of obſervation. The computation would be founded on the following problem, which will not preſent any difficulty to thoſe who are acquainted with theſe ſubjects, if the phyſical allowances for temperature in barometrical admeaſurements be admitted to be correct enough for this purpoſe.

Perpendicular teleſcope by which their courſe and velocity were ſeen on the ground.

Inveſtigation of the degree of accuracy of this method.

† Or rather, perhaps, becauſe not exactly in the ſame current of the atmoſphere. N.

Given

Problem for computing the velocity of a balloon by obs. through a telescope.

Given the temperature on the earth and in the car, the height of the barometrical column, and the time employed in the apparent transit of an object on the earth through a given angle or field of view; to find the velocity of the observer.

Tabulated results for practice.

For practice it would perhaps be sufficiently exact and convenient to compute a small table, in which, neglecting the temperature, the velocity in miles per hour might be had by inspection, when the height of the mercury, the time of transit, and the magnifying power were known.

The observed course, &c.

The aeronauts having noticed by their instruments what were the direction of the currents of air at different heights, found themselves in one which carried them directly towards the Baltic. They therefore descended till they saw by the barometer that they had returned to a current which carried them inland; and afterwards again rose much higher, and saw with great precision by their telescope the instant of their quitting the gulph. When the barometer stood at 24 inches they let go a pigeon, who flew with difficulty and would not quit the balloon; but upon being precipitated he in vain endeavoured to regain it, and at length descended rapidly towards the earth.

A pigeon let go.

The dip of the magnetic needle is less at great heights in the air,

At ten at night the balloon had risen to an height indicated by 22 inches of the mercurial column, the thermometer standing at 4½ degrees (I suppose centigrade). Here it was that M. Sacharoff carefully observed a phenomenon which had been before remarked by M. Robertson in his ascent from Hamburgh, but at a much greater elevation. Their dipping needle was deranged; but on inspecting the common compass, its needle was found to be no longer horizontal, the north end being elevated near 10 degrees. On this phenomenon they remark that the magnetic attraction probably diminishing as the square of the distance may afford additional means of directing future observers in the atmosphere, and even determine the elevations independently of the barometer. From the present elevation a pigeon being thrown down, fell so directly that it was doubted whether he could have reached the earth alive.

This may afford means of shewing the heights, &c.

Singular echo of the voice heard from the earth at the distance of two miles.

Darkness coming on, it became necessary to descend, during which the observer repeatedly made an experiment which also promises to be of great utility to voyagers in the air, as well as to enlighten our conclusions respecting the phenomena of sound. When they spoke through a trumpet directed towards the earth, the voice was returned with extreme precision and without

without feeming to have loft any part of its intenfity. No repetition was made except when the trumpet was directed to the earth; and the intervals of reflection were different according to the elevation of the obfervers. The percuffion impreffed on the air by the found every time produced a flight undulation in the aeroftat; whence they deduce an inference in favour of the fuppofed efficacy of cannon in partly modifying or averting the difcharge of ftormy clouds. In one of their experiments, the found employed ten feconds in its return, which would give a diftance of about two miles out and home, if the fame law of the velocity of found were fuppofed to prevail in the perpendicular courfe as along the furface of the earth, which however does not feem likely. The barometer ftood then at 27 inches, and at their outfet it was at 30 inches on the ground. It would be eafy, and it is furely defirable to make experiments with cannon and ftop watches on the velocity of afcending, and if poffible, defcending found.

No echo but when the trumpet was directed to the earth.

This reflection of found or echo is a fubject of very great curiofity. There is perhaps no other inftance in nature where fo extended a wall of reflection can be had. I am difpofed to think that the apparent intenfity of the returned found may in fome meafure have depended on the perfect filence in which the fpeakers were placed. In a ftill night the centinels on the ramparts at Portfmouth may be heard at the Ifle of Wight, over a diftance of five miles, and there are numerous inftances of low founds, fuch as the beating of a clock or watch, or the founds of footfteps being heard to confiderable diftances, when other founds do not act on the organ of fenfe.*

The ftrength of the found perhaps depended on the ftillnefs of the fituation.

In their defcent to the earth they paffed through various ftrata of vapours, all of different temperatures, and at the inftant the earth came in fight, the thermometer ftarted up through feveral degrees, probably becaufe they had quitted a cold mafs of vapour which obfcured their view, or perhaps becaufe the radiant heat of the earth's furface might at that moment have reached them unimpaired.

Sudden rife of the thermometer in defcending.

* See a curious paper of M. Perrole on found, with the annotations thereon, in the firft Vol. of our quarto Journal, page 411.—N.

Letter

XIII.

Letter from Mr. CUTHBERTSON, *containing Remarks on Mr.* WM. WILSON's and *Mr.* HAUY's *Experiments on the Electricity of Metals.*

To Mr. NICHOLSON.

London, 54, *Poland-Street*,
DEAR SIR, *April* 24, 1805.

Mr. Wilson's experiments on electricity;

IN your valuable Journal for January, page 42, you have favoured us with a letter from Mr. Wilson, containing some experiments which he calls, *exhibiting the electricity of metals.*

of copper sifted through zinc,

Mr. Wilson says in his table of experiments, that copper filings sifted through zinc is positively electrified: this is an *error* which I thought myself obliged to take notice of, as it is a direct contradiction to what I have asserted in *my examination of Sig. Volta's experiments, which he calls fundamental, and on which his theory of galvanism rests; see this Journal, vol.* II. *page* 281; I have said therein, that if a plate of zinc be separated from a copper one, it will be positive, consequently leave the copper negative if insulated: whether this experiment be performed as I have therein mentioned, or according to Mr. Wilson, the one reduced into filings, and afterwards sifted through holes made in the other metal, the electricity excited must fall under the same denomination, the difference will be that the quantity of electricity excited will be more by Mr. W.'s method than mine, because he has multiplied the separations, a fact well worth notice.

controverted by another experiment of contact and separation.

Contact and separation produce the electric state;

Mr. W.'s chief object in view seemingly, is to prove that the separation of metals is the cause of the electric fluid being excited and not touching. I cannot perceive that these experiments throw any light upon that subject, as both touching and separating are here employed.

but neither act will do alone.

In my second volume on electricity, published in Amsterdam, anno 1782, I have proved by experiment that neither *touching* nor *friction* separately employed excites electric fluid, friction and separating jointly employed, is a powerful exciter of electricity on glass; touching and separating jointly employed on glass excite electric fluid in a slight degree, and only when the state of the atmosphere is favourable.

In

In the Philosophical Magazine for Nov. 1804, page 120, we have Mr. Hauy's observations on the electricity of metallic substances: he does not inform us what shape his *silver* pieces were, or whether they were pure or with alloy; he however does not hesitate in pronouncing of it to be positively electrified by friction; so that it does not seem that he has entered very wide into the subject, or he would have perceived some remarkable changes to take place in that metal by friction, and particularly in coins. They will change from positive to negative, and *vice versa*, without any visible cause. If a dollar and a half-crown be stuck to the ends of two sticks of sealing-wax, and rubbed separately upon woollen cloth, they will be found, after the friction, sometimes positive and sometimes negative, and sometimes one positive and the other negative, without varying the manner of friction. If pure silver, or silver with different proportions of alloy, be melted down to a button, and used in that shape, or hammered flat, representing coins, they are for the most part positive by friction. These experiments upon metals are not new; I believe they were first begun by Mr. Henly, and inserted in the Philosophical Transactions; but I have not the data at hand, and I do not not remember that he had observed the above-mentioned property of this metal.

Hauy's obs. on metallic substances, imperfect.

Singular effect of rubbing coins, &c.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Note on the New Planet Juno.

I HAVE not had any late account of the new planet announced at page 301 of our 9th volume (Dec. 1804) in a letter from M. Bode. The discoverer's name, who is Mr. Harding of Lilienthal, near Bremen, was not then mentioned.—For the present I give the following notes from the Journal de Physique, Thermidor last.

Planet Juno.

On the 5th of September, 1804, its right ascension was 1° 52′; declination 0° 11′ north. M. Burckhardt observed it on the 23d of September at 359° 7′ and 4° 6′, whence he concludes that the duration of its revolution is five years and a half. Its inclination 21°; excentricity one quarter of its radius; mean distance from the sun three times greater than that of the earth.

Its

Its diameter could not be measured, but it appeared like a star of the eighth magnitude. It seems nearly equal to that of Ceres, or the planet discovered by Piazzi.

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*Discovery of Fluoric Acid in the Topaz.**

Saxon topaz analysed by Vauquelin. Brasilian by Descotils.

IN the year 1797, M. Vauquelin analysed the Saxon topaz, and found its constituent parts to be, silex 31, alumine 68. Mr. Descotils soon after examined the Brasilian topaz; but as there was a loss of 18 *per cent.* in his first analysis, and 12 in his second, he did not think fit to publish the results of his labours at that time; and other circumstances prevented him from pursuing the inquiry, as he had intended.

Klaproth discovered fluoric acid in the Saxon topaz.

Not long ago Mr. Klaproth wrote to Mr. Hauy, that he had found fluoric acid in the Saxon topaz. Mr. Laugier made several experiments to verify this discovery, but without success. It is true in the analysis with potash he found a deficiency of 16 per cent. but though he reduced the topaz to an impalpable powder, and did all he could to expel the fluoric acid from it by means of the sulphuric, he was unsuccessful. Mr. Vauquelin on his return applied himself to the same research, and we here present the result of his labours. Not knowing what process Mr. Klaproth had employed, he tried that which seemed to him most likely to succeed. He first heated the topaz with caustic potash in a silver crucible in the usual way. After he had diluted the mass with water, he introduced it into a retort, and poured on it sulphuric acid. White fumes soon arose, and these, being collected, exhibited all the characteristics of fluoric acid combined with silex. The latter came almost wholly from the stone, as the retort was not perceptibly attacked by the acid.

Sulphuric acid incapable of expelling it.

Vauquelin treating the topaz with potash and sulphuric acid expelled the fluoric.

The Brasilian topaz gives the same result.

The same experiment with the Brasilian topaz gave the same result; and there is every reason to believe, that the Siberian, which Mr. Vauquelin is now analysing, will afford the same products. Thus we may now consider this gem as a siliceous compound, consisting of fluoric acid, alumine, and silex, or a true alumino-siliceous fluate; and the discovery must be considered as of the highest importance in mineralogy.

Topaz a true alumino siliceous fluate.

* Bulletin des Sciences, No. XC. p. 282, Sept. 1804.

Mr.

Mr. Vauquelin next inquired what might be the circumſtance, that led him into an error when he firſt analyſed the topaz; and he imagines, that it was his treating the alcaline maſs with muriatic acid, inſtead of employing the ſulphuric; and that probably not heating it to a ſufficient degree to expel the fluoric acid, from fear of decompoſing the muriate of alumine, he precipitated the fluoric acid combined with the alumine, when he added ammonia to ſeparate the alumine from its muriatic ſolution. Circumſtance that miſled Vauquelin in his firſt analyſis.

Examination of Crude Platina, by Meſſrs. TENNANT and WOLLASTON.*

THE editors of the *Annales de Muſeum* obſerve, that platina, when taken from the mine, appears from the experiments of Deſcotils, Fourcroy, and Vauquelin, to be mixed with iron, chrome, and other metals, among which may be one or two that are new. Mr. Tennant has given names to two, and Dr. Wollaſton to two others; but the French journaliſts congratulate their countrymen for being leſs haſty, and waiting till they are aſſured theſe new metals really exiſt. Platina.

Prize Queſtions in France.

The Society of Agriculture and the Arts, in the department of the North, propoſes for the ſubjects of two prizes, which ſhall be adjudged, the firſt in the firſt of Fructidor, in the year XIII. and the ſecond in the firſt fortnight of Fructidor, in the year XIV. the two following queſtions: Prize queſtions reſpecting wool; and an inſect on the colſa.

Firſt Queſtion.—" What method of propagating, rearing, feeding, and houſing the ſheep of the race now exiſting in the department of the North, ought to be followed in this department, to obtain wool from theſe animals, equal in quality to the beſt wools from Engliſh ſheep?"

Second Queſtion.—An inſect known in the country under the improper denomination of *puceron*, has this and ſeveral preceding years attacked and deſtroyed the greater part of the flowers of the colza.—" What is this inſect? Under what generic and ſpecific name have the moſt celebrated naturaliſts deſcribed it? What

* Bulletin des Sciences, No. XC. p. 234, Sept. 1804.

is

is its life, either in the state of coleoptera in which it is found on the colza, or in the state of larva? What natural enemies, what artificial means of destruction can be opposed to it in either state? Generally, what care can preserve the colza from its ravages?"

Each of these two prizes will be a gold medal of the value of 150 francs.

Russian Circumnavigation.

Russian voyage round the world.

By a courier, expedited by the governor of Kamschatka, news was received at Petersburgh on the 21st of December, that the vessels belonging to the expedition round the world under the orders of M. de Krusenstern, had arrived, on the 26th of July last, at the harbour of Peter and Paul, in Kamschatka, and that, up to this time M. Krusenstern had not lost a single man of his retinue, nor had he any sick in the squadron. In his voyage he had visited the Marquesas and Sandwich Islands, M. Krusenstern purposed sailing for Japan, towards the end of August.

Europeans found at the Marquesas, who had forgotten their own language.

At one of the Marquesas, where the vessels stopped a few days, he had taken on board a Frenchman and an Englishman, to bring them back to Europe. Captain Krusenstern says he had not yet been able to discover how these two individuals had come to this island; both of them having completely forgotten their original language. He thought however that he could comprehend that they had arrived there on board an American vessel which had been shipwrecked on the coast. The Frenchman speaks the language of the islanders very well, and has adopted all their customs, habits and manner. There is no doubt that they will soon recover the use of their language, in a daily intercourse with Europeans; and that they will then be able to give an account of their adventures, as well as information respecting the islanders, among whom they have lived so long. At least, this is expected with impatience.

Geological Journey from the Academy at Warsaw.

Journies of discovery in mineralogy and natural history.

The Society of the Friends of the Sciences at Warsaw have charged two of its members, M. Carteau and Stacio, to undertake a mineralogical and physical expedition into the Carpathian mountains. Another member of the society has already examined

examined the eaſtern part of them, with relation to mineralogy, geogony and oryctognoſy. At this time he is travelling over the mountains in the interior of Auſtria, from whence he will proceed to upper Italy, and to the Alps of Switzerland. When this journey is completed, he will undertake another to Caucaſus.

Lalande's Propoſal of a new Scale of the Thermometer.

New ſcale of thermometer.

M. de Lalande propoſes to adopt a thermometer-ſcale, which ſhall remedy all the inconveniences of thoſe now in uſe. His mean point is taken from the natural ſtate of the globe, which he fixes at 9½ degrees of Reaumur's thermometer, and he takes the 10 millionth part of the volume of mercury for the meaſure of a degree. Among the advantages of this inſtrument he reckons a ſimplification of expreſſion, which will give a facility to comprehending what was before without meaning to the public. For example, the degree of heat of common ſummers, and the degree of cold of our mean winters, will be both expreſſed by 30: The degree of 40 will indicate a hot ſummer and a ſevere winter, &c.; another advantage will be derived from the ſmaller interval of the degrees, which will remove the neceſſity of having recourſe to fractions in the greater number of obſervations. The boiling point of water is + 132°.8 of the propoſed thermometer, and − 74°.4 is the point of the congelation of mercury. Ice melts at − 17°.9 and − 44°.2 is the zero of Fahrenheit.

Two Kinds of Honey.

Prouſt on honey.

In a note to Dr. Delametherie, Prouſt announces the diſcovery of two kinds of honey; the one liquid, the other dry, not deliqueſcent, cryſtallizable in its manner, and leſs ſaccharine than ſugar: they are ſeparated by ſpirit of wine, to which end granulated honey muſt be operated on.

J. de Phyſique.

Palladium.

Attempts to form palladium with mercury and platina.

Experiments made by Meſſrs. Roſe and Gehlen, and others by Richter, to obtain palladium, are given at length in the Journal of Chemiſtry, publiſhed in German by Klaproth and Richter.

Theſe

These philofophers followed Mr. Chenevix's procefs with great care, but did not produce that metallic body. In the precipitation of muriate of mercury and muriate of platina, they had a black powder, which always afforded the metals feparate from each other.

Richter was not more fuccefsful. He verified that the green fulphate of iron does not decompofe either the muriate of mercury or that of platina. The other facts he obferved were to the fame effect as thofe of Rofe and Gehlen. He always found the mercury of his precipitate feparated from the platina by heat.

Traité élémentaire d'Hiftoire Naturelle, &c. *An elementary Treatife on Natural Hiftory, by* A. M. CONSTAT DUMERIL: *Compofed by Order of Government for the Ufe of the National Lyceums,* 1 *Vol.* 8vo. *Paris* *.

Elementary works on natural hiftory very defective.

There is not one of the fciences, the elementary works of which have been fo long neglected as natural hiftory. Sometimes this title has been given to collections of tales fit only to amufe children, but not calculated to make them acquainted with nature as a whole, and with the progrefs of the fcience: at other times authors have entered into difcuffions too abftrufe, or contented themfelves with a mere nomenclature, always dry and fterile to beginners, to whom names give no idea of objects with which they are unacquainted. Mr. D. has preferved a juft medium between thefe extremes, while he obferves an accurate and methodical arrangement. He makes us acquainted with the whole of the productions of nature, and the method of ftudying and claffing them, choofing for examples in every fection fuch as are moft remarkable for their ufes or fingularity; he continually excites the attention and curiofity of his pupils; and he prefents to them a number of facts neceffary to be known.

Mr. D. has purfued a better method.

His arrangement proceeds from the moft fimple things to the more complex.

In this work Mr. D. has adopted an arrangement, the reverfe of what is ufually employed in books of natural hiftory; that is, he always proceeds from the moft fimple to the moft complex. He begins with unorganized fubftances, proceeds hence to plants, and laftly to animals; and in thefe he commences

* Bulletin des Sciences, No. 90, p. 236, Sept. 1804.

with

with zoophytes, and ends with man. This arrangement has the advantage of inſtilling ideas gradually into the mind of the ſcholar, and avoiding a number of repetitions and anticipations. The hiſtory of organiſed ſubſtances gives him an idea of bodies unmixed with any other ideas: that of vegetables ſhows him organization and life in their moſt ſimple ſtate: and theſe he perceives gradually become more complicated as he aſcends through the different claſſes of the animal kingdom, ſo that the hiſtory of each claſs is but little more than an expoſition of the organs and faculties it enjoys beyond thoſe of the preceding. Its advantages.

Though the diſcuſſion of any new idea ſeems contrary to the eſſence of an elementary work, it is obvious, that ſuch a work cannot be well executed but by a man capable of conſidering the whole of a ſcience in its proper point of view. In this reſpect the naturaliſt will here read with pleaſure the article of general obſervations placed at the head of each part: he will diſtinguiſh the hiſtory of inſects, which Mr. D. has treated after a new plan: and he will notice the chapter on man, in which the author diſplays the phyſical characters that diſtinguiſh man from brutes, and the conſequences reſpecting his manners that ariſe from his very ſtructure. This chapter may be conſidered as the connecting link between phyſics and metaphyſics. Mr. D. has treated the inſects on a new plan.

Elemens de l'Art de la Teinture, &c. *or Elements of the Art of Dying; with a Deſcription of the Proceſs of Bleaching by the oxigenated Muriatic Acid. Second Edition: by* C. L. *and* A. B. Berthollet. *Paris.*

This new edition of a work of the firſt merit and celebrity is ſpoken of in the Foreign Journals, as being conſiderably improved by the former author and his ſon. The great perſpicuity and order which are ſeen in all the productions of this eminent chemiſt, and his own unremitted labours in the ſcience, are a ſure guarantee to the ſame effect. Berthollet on dying.

Conſiderations on Organized Beings. By J. C. Delametherie.

The ſcience of natural hiſtory is indebted to M. Delametherie for ſeveral intereſting works which contain ideas of advantage to the progreſs of human knowledge. Beſides the Delametherie on organized beings.

5 Journal

Journal de Physique, &c. of which he is the editor, he has published a Theory of the Earth; a Treatise on Man; different Physiological Views of the animal and vegetable Kingdoms, on Vital Air, &c. In this new work the author compares the structure of animals and plants, and applies to vegetables the different systems or apparatuses of organs and vital functions, which X. Bichat has proposed in his General Anatomy. M. Delametherie has benefitted by the experiments and labours of several philosophers of merit. All, perhaps, may not adopt some of the opinions he offers, the conquest of minds being still more difficult than that of hearts; but they, at least, deserve examination, and may lead to unexpected results.

Bibliothcque de Sonini.

To CORRESPONDENTS.

My best acknowledgements are due to Mr. D. *who has favoured this work with a precise statement of the elucidation of Mr.* BOSWELL'*s second proposition; but he will perceive that the paper of Mr.* GOUGH, *which was already printed when his letter came to hand, has rendered it unnecessary.*

I believe the readers of this Journal will think with me, that the dispute between C. L. *and Mr.* E. WALKER *has proceeded at least as far as the interests of science demand. In a letter from* C. L. *before me, I have supposed the following explanatory sentences to afford no new ground for discussion, and therefore extract them, and hope the business will end here.* C. L. *says:*

"*I cannot conceive how expressions so plain could be misunderstood, and hope you will permit me to endeavour to make them plainer if possible.* I have denied Mr. WALKER'S facts; *that is, I have denied the* truth of *his narrative respecting* certain supposed facts, *and I have pointed out an easy way of convincing me and the world that he is not deceived; namely, that you should repeat his experiments, and see if what he asserts be true. But Mr.* WALKER declines examining the remainder of my letter, *which is an indistinct, though sufficiently clear way of saying, that he does not choose to risk his supposed facts by putting them to such a trial.*"

Mr. Dodd's Gun Lock.

Fig. 3.

Fig. 1.

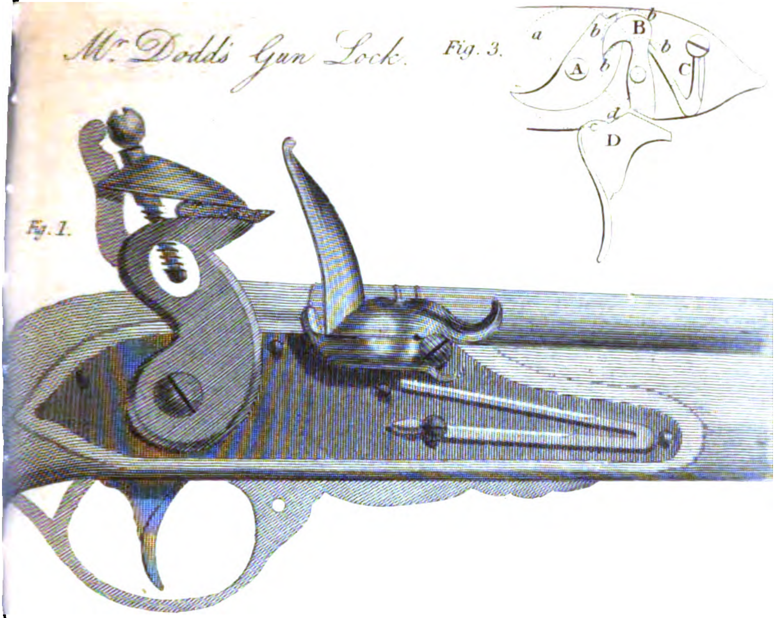


Fig. 4.

Fig. 2.

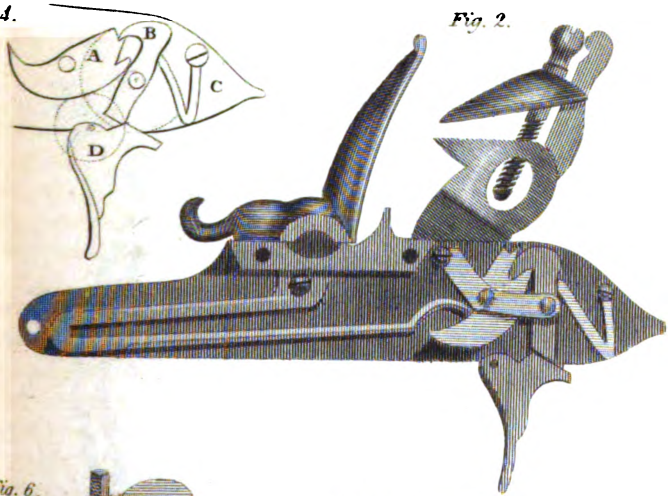
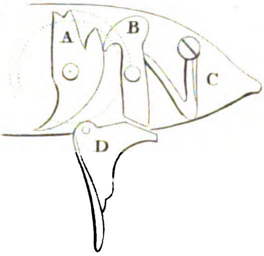


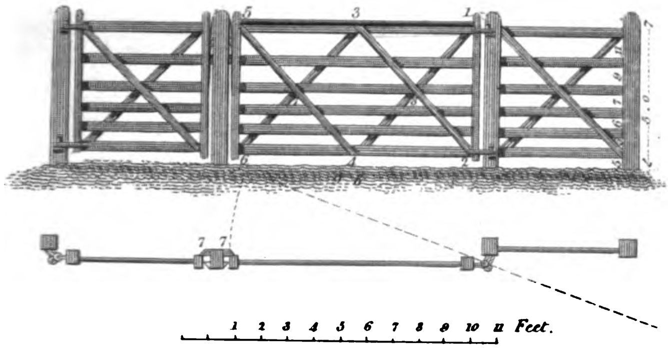
Fig. 6.

Fig. 5.



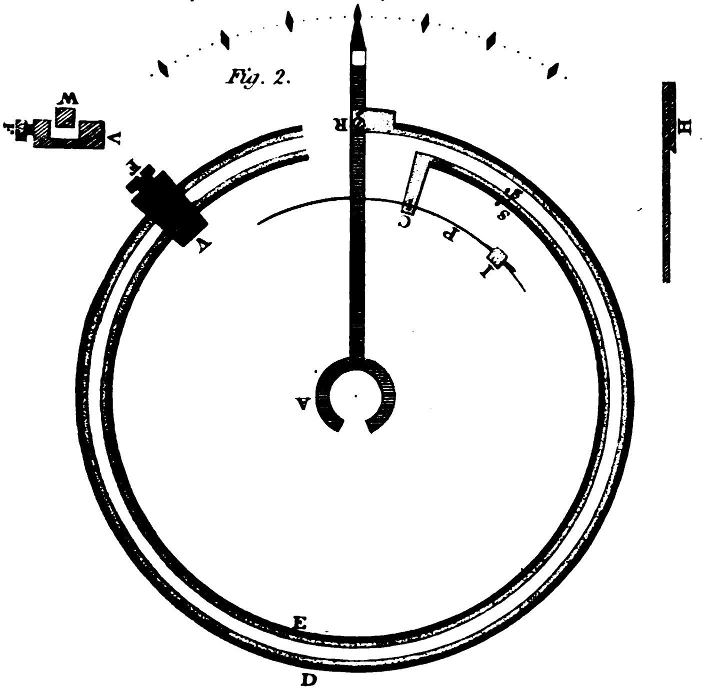
Mr. Waistell's improved Field Gate.

Fig. 1.



Mr. Scott's compensation Curb for a Time piece.

Fig. 2.



Mutlow Sc. Russell Ct.

Bavarian Method of evaporating Salt Waters.

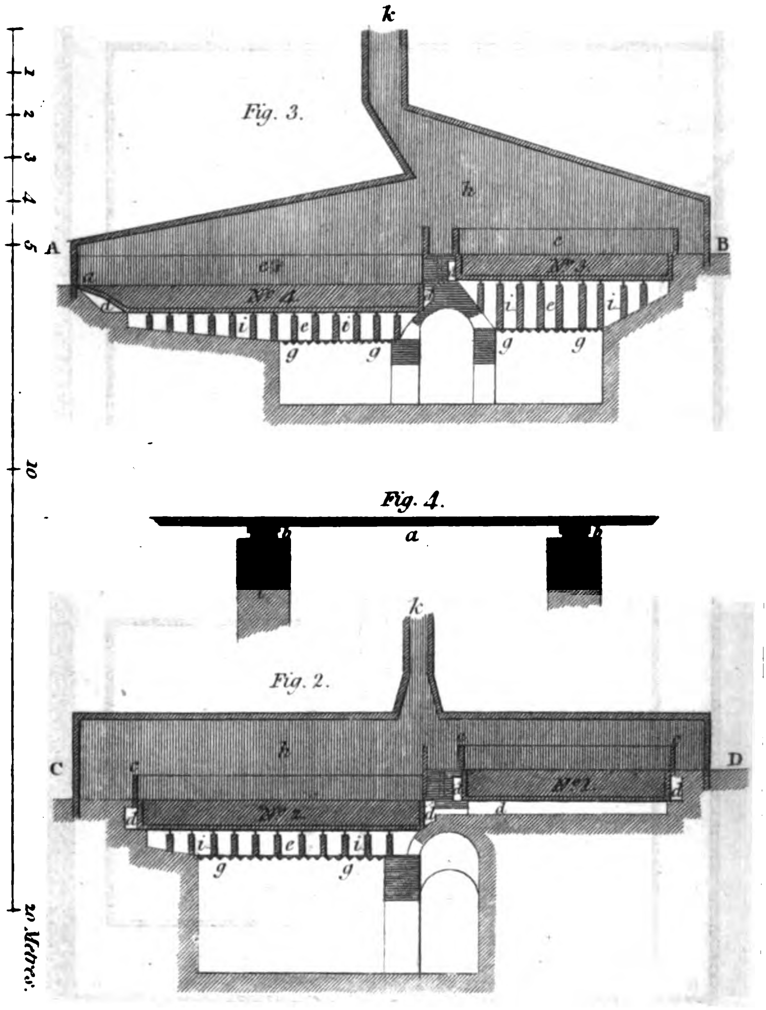
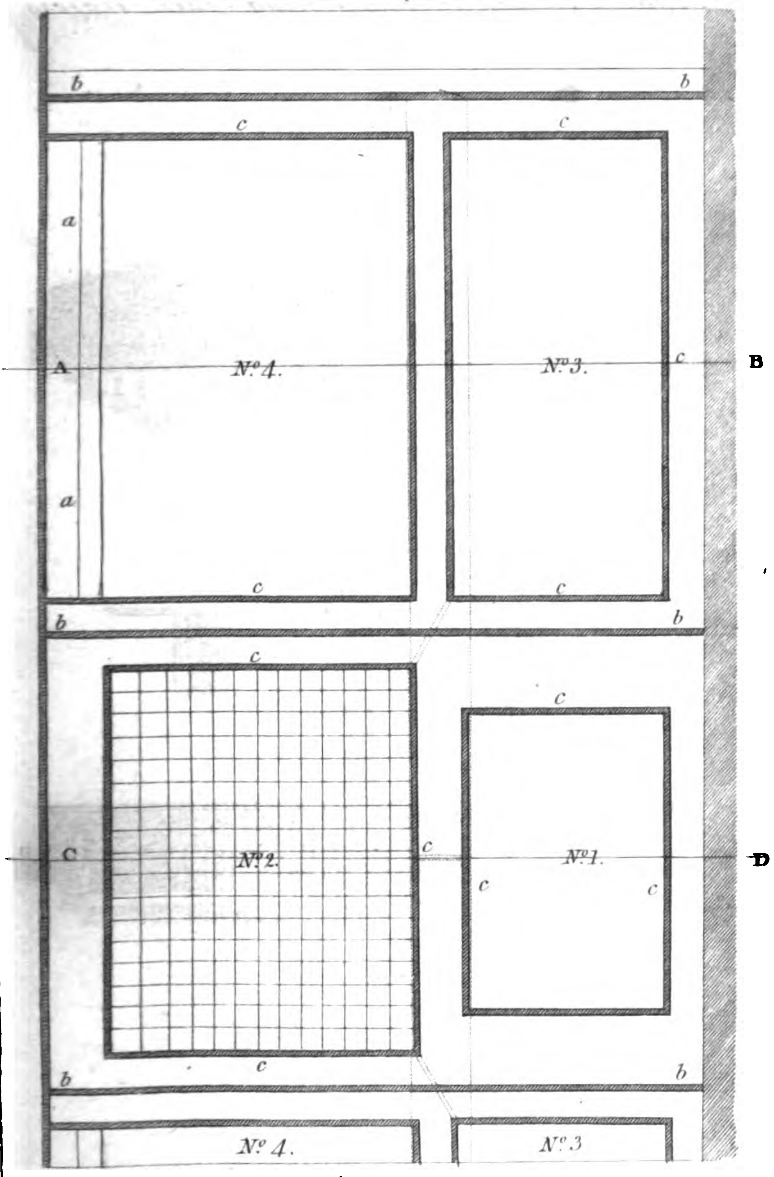


Fig. 1.



Mutlow Sc. Russell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1805.

ARTICLE I.

Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combustion, when employed as Sources of artificial Light; aud including Observations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. William Henry. *(Communicated by the Author.)*

THE gas obtained by the destructive distillation of pit-coal, has become an object of considerable interest and importance, in consequence of its successful application (by Mr. Murdoch, of Soho, near Birmingham *) to the purpose of affording light. Having constructed an Argand's lamp last winter, with the view of effecting the combustion of the gas on Mr. Murdoch's plan, I made previous trials with pure hydrogen gas, with carburetted hydrogen obtained by passing water over ignited charcoal, and with the carbonic oxide; but found that each of them burned with so trifling a production of light, as to be altogether unfit for the purpose of illumination; while the light evolved by the gas from coal was little, if at all, inferior to that

Process of Mr. Murdoch to illuminate by gas from coal.

Neither hidrogen nor carbonic oxide give any notable light; but the gas from coal is nearly equal to oil.

* See the statement of Mr. M's claim to the discovery in the postscript.

that from good ſpermaceti oil. So eſſential a difference in the combuſtion of theſe gaſes, induced me at firſt to believe, that the gas from coal owes its illuminating property to ſomething mechanically ſuſpended in it; but I was afterwards ſatisfied, that though it contains, when recently prepared, much that is ſubſequently depoſited, yet that its quality of burning with a bright and compact flame belongs to it, though certainly with conſiderable diminution, as a permanent gas, after the ſeparation of all condenſible matter. It appeared, therefore, worthy of inveſtigation to determine, on what the ſuperior fitneſs of the gas from coal, for evolving light, depends; and to connect the theory of its combuſtion with that of other ſubſtances, commonly employed as ſources of artificial light.

This laſt gas burns with much (though diminiſhed) light after it has depoſited its condenſible matter.

Products by burning the gas from coal with oxigen and alſo other gaſes.

With this view, numerous comparative experiments were made on the rapid combuſtion of this gas with oxigenous gas in cloſe veſſels, and alſo on that of other inflammable gaſes; and their compoſition may be inferred from the products of theſe experiments, the principal reſults of which are contained in the following table:

Table of reſults.

Kind of Gas.	Oxigen Gas required to ſaturate 100 Meaſures.	Carbonic Acid produced.
Pure hydrogen,	50 to 54	
Gas from moiſt charcoal,	60	35
——— wood (oak)	54	33
——— dried peat,	68	43
——— coal, or cannel,	170	100
——— lamp-oil,	190	124
——— wax,	220	137
Pure olefiant gas,	284	179

If the meaſure of carbonic acid produced, be deducted from the whole oxigen employed, the remainder will expreſs the meaſure of oxigen which was employed in burning the hydrogen of the gas; and this laſt doubled will give the

Now if it be aſſumed (which I believe is as nearly as poſſible the fact) that in the formation of each meaſure of carbonic acid, in the above experiments, an equal volume of oxigen gas is employed*, we ſhall learn, by deducting the numbers in the

* Mr. Cruickſhank takes it for granted, as the baſis of his calculations, that in forming ſix meaſures of carbonic acid, ſeven meaſures of oxigenous gas are employed. This proportion I believe to be over-eſtimated. Dr. Prieſtley obſerves, (on Air, 2d Edition, III. 377), "I heated 8½ grains of perfect charcoal in 70 ounce meaſures of dephlogiſticated air, when it ſtill remained 70 oz.

m.;

the third column from the corresponding one in the second, what proportion of the consumed oxigen has been allotted to the saturation of the hydrogen of each hydro-carburet. Thus, for example, in the combustion of the gas from coal, 70 parts of oxigen have disappeared, besides that which has entered into the carbonic acid; and, since each measure of oxigen saturates two of hydrogen gas, the gas from coal must contain, in 100 measures, a quantity of hydrogen which, expanded to its usual elasticity, would occupy 140 measures. By a similar mode of estimation, the quantity of hydrogen in other species of inflammable gas may be ascertained; *viz.* by subtracting the number in the third from the corresponding one in the second column, in each instance, and doubling the remainder.

measure of hydrogen originally present.

The above experiments sufficiently explain why the gas from coal evolves so much more light, during combustion, than either hydrogen or the hydro-carburet from moist charcoal, because, in an equal volume, it includes, in its composition, above thrice the quantity of inflammable matter present in hydrogen gas, and nearly thrice as much as is contained in the gas from moist charcoal. The appreciation of the degree of combustibility of each gas, by the quantity of oxigen required for its saturation, entirely agrees, as might naturally be expected, with that founded on the phenomena of silent combustion in an Argand's lamp; for each gas seemed to me to evolve light, as nearly as could be judged, in proportion to the quantity of oxigen consumed by its detonation in a close vessel. Above all others, the olefiant gas * is decidedly entitled to rank, by the splendor and beauty of the light which it yields; and the violence of its detonation, when fired with a mixture of oxigen gas, also surpasses that of every other inflammable gas. By exploding only .03 of a cubic inch with .17 of oxigen gas, a strong glass tube was

These facts explain the greater illumination from the coal gas. The light is greater the more oxigen is required for the combustion.

m.; but, after washing in water, was reduced to 40 oz. m." In this experiment one grain and ½ of charcoal was consumed, and 30 oz. m. of carbonic acid were generated, without any change in the volume of the oxigenous gas.

* A full abstract of the memoir of Messrs. Deiman, &c. on this interesting gas, may be seen in the 1st Vol. of the 4to Series of this Journal. Its characteristic property is that of being rapidly condensed into oil, by contact with oxigenized muriatic acid gas.

 shattered

shattered with violence; and a Volta's eudiometer, $\frac{1}{4}$ of an inch in thickness was burst by less than a cubic inch of a mixture of the two gases.

Specific gravity of an inflammable gas (freed from carbonic acid) is a test of its fitness to afford light.

The specific gravity of the inflammable gases, when perfectly freed from carbonic acid, is another competent test of their fitness as sources of light. Thus the specific gravity of the gas from moist charcoal, (common air being 1000) according to Cruickshank is 480; of the hydro-carburet from alcohol 520, and of the olefiant gas, as determined by the Dutch chemists, 909.

It is probable that the inflammable gases are mixtures of few simple gases.

From the limitation to the proportions, in which bodies in general, having a susceptibility of chemical union, are capable of combining, it seems to me reasonable to infer, that carbon and hydrogen do not unite in all possible proportions, forming so many distinct compounds; but that the various inflammable gases are mixtures of a very few simple ones. Of those at present known, pure hydrogen gas; the carburetted hydrogen, which by combustion affords an equal bulk of carbonic acid, and consumes twice its bulk of oxigen; the carbonic oxide; and the olefiant gas, it will appear, may be traced in the mixed gases comprehended in the forgoing table.

Gas from coal appears to be hydro-carburet with perhaps some carbonic oxide.

The gas from coal I apprehend to be principally hydro-carburet, with perhaps some portion of carbonic oxide, the presence of which last is rendered probable, because the gas from coal is saturated by less than twice its bulk of oxigen, though it gives an equal volume of carbonic acid. Now the gas from marshes, which, with Mr. Cruickshank, Mr. Dalton finds to be hydro-carburet, contaminated with about 20 *per cent.* azotic gas, consumes, making allowance for this adulteration, double its volume of oxigen gas; and since the gas from coal requires a less proportion than this of oxigen, and yet gives an equal product of carbonic acid with that from marshes, it is fair to presume, that it must previously have contained some oxigen, which, after washing it with lime-water, can subsist in no other state than that of the carbonic oxide.

Gas from ignited charcoal and water is probably carbonic oxide with hydrogen and a little hydro-carburet.

The gas obtained by decomposing water over ignited charcoal, is most probably a mixture of carbonic oxide with hydrogen gas, and perhaps a little hydro-carburet. On no other presumption can the results of its combustion be explained; since the quantity of oxigen required in saturating 100 measures is only ten more than are consumed by 100 measures of pure hydrogen, though 35 m. of carbonic acid, containing at least

35 of

35 of oxigen, are found after combuftion. Now, according to Mr. Cruickfhank, thefe 35 m. of carbonic acid, if formed from carbonic oxide, would require only 15 additional meafures of oxigen gas, which is not very remote from the truth. Reafoning in the fame mode, the gafes from wood and from peat will appear to be mixtures in different proportions of two at leaft of the above-mentioned, viz. hydrogen, and the carbonic oxide. The refults of the combuftion of the gafes from wood and peat evince that they differ confiderably from that obtained from coal; and contain much lefs uncombined inflammable matter. Another circumftance of diftinction alfo is, that, before being wafhed with lime-liquor, the gas recently prepared from wood or from peat, contains from $\frac{1}{4}$ to $\frac{1}{3}$ its bulk of carbonic acid; whereas the gas from coal lofes by this ablution only from $\frac{1}{27}$ to $\frac{1}{13}$. * In my firft experiment, I found a large admixture of azote in all thefe gafes; but this afterwards proved to be accidental and not effential; fince by careful diftillation in glafs retorts, of the fubftances that afforded them, the gafes were obtained entirely free from this contamination.

Gafes from wood and from peat contain lefs of uncombined inflammable matter than the coal gas.

The gafes obtained by the deftructive diftillation of oil and of wax, it may be obferved in the table, confume confiderably more oxigen than the gas from coal. This circumftance firft led me to fufpect that they might poffibly be mixtures of the olefiant gas with carburetted hydrogen; and on applying the oxygenized muriatic acid gas, this fufpicion was fully verified. One meafure of the gas from oil with one of the oxygenized gas, were reduced fpeedily to $1\frac{3}{4}$; a like diminution was produced in the gas from tallow; and that from wax had its bulk ftill further contracted, only $1\frac{1}{2}$ m. being left by fimilar proportions.

The gafes from oil and from wax are pure hydro-carburets, except that the firft contains one-eighth and the latter one-fourth of olefiant gas.

* The condenfible products alfo of coal probably differ from thofe of wood and peat. If an intermediate veffel be placed for their reception, it emits, after the diftillation, a ftrong fmell of ammonia. This was long ago obferved by Lord Dundonald, who enumerates, among other products of coal, the volatile alkali (fee a pamphlet "on the Ufes and Qualities of Coal Tar," publifhed by his Lordfhip in 1785.) This production of ammonia I have not obferved from peat or wood; nor do I find it mentioned in a Hiftory of Peat, including the refults of its diftillation, &c. publifhed in the fecond vol. of the Edinburgh "Effays Phyfical and Literary."

In order to ascertain how much of the diminution was owing to the condensation of olefiant gas, the proportion of oxygenized acid, required for the saturation of that gas, was carefully ascertained. After several trials, it was found that 3 m. of the oxygenized acid with 2½ pure olefiant gas, prepared according to the Dutch chemists, left only 0.15 m. of common air derived from the vessels. It appears, therefore, that the gas from oil and from tallow contains about ⅛, and that from wax ⅙ olefiant gas, the rest being pure hydro-carburet.

Hence it is seen why washing diminishes the product of carbonic acid afforded by burning an hydrocarburet. For it takes away the olefiant gas.

The results of these experiments, in connection with a fact communicated to me by Mr. Dalton, explain some circumstances observed by Mr. Cruickshank, for which that ingenious chemist was at a loss to account, viz. the great variation in the products of carbonic acid, obtained by burning the same hydrocarburet when washed and when unwashed, or when long kept in contact with water; though the gas, when originally procured, was perfectly free from carbonic acid. The olefiant gas, Mr. Dalton has ascertained, is far more absorbable by water than other species of hydrocarburet, viz. in the proportion nearly of ⅛. Now the gas from camphor, I find to contain much olefiant gas, and indeed this might have been inferred from Mr. Cruickshank's own statement, who observes that this gas, by admixture with oxygenized muriatic acid, undergoes a considerable diminution of bulk. The pure hydrocarburet, on the contrary, I have never seen at all condensed by contact with this gas, in the rapid manner observable in olefiant gas or mixtures containing it; though, by confinement together for some hours, they are mutually decomposed into common muriatic acid, carbonic acid, and water. The hydrocarburets from ether and alcohol also contain olefiant gas; and this, I apprehend, will be found to be the fact with all inflammable gases, which by combustion give more than their own bulk of carbonic acid. The variable products of carbonic acid, obtained in Mr. Cruikshank's experiments, from equal quantities of different hydrocarburets, cannot, therefore, be considered as denoting so many distinct species of carburetted hydrogen; but as owing to the admixture with this of various proportions of olefiant gas.

Hydrocarburets from ether and alcohol also contain ol. gas; which is the general cause of the difference in carb. acid products by Mr. Cruikshank.

Error of that author respecting the constitution of carburetted hydrogen gases.

It will not, I am persuaded, be regarded as indicating a wish to detract, in the smallest degree, from the credit due to Mr. Cruikshank, whose memoirs on the hydrocarburets and carbonic

carbonic oxide I estimate among the most ingenious, and generally speaking, the most satisfactory examples of chemical research, if I observe that the part of his table (vol. V. p. 8 of the 4to series of this Journal) which relates to the constitution of the carburetted hydrogen gases, I consider as entirely erroneous. To excite strong suspicion of the accuracy of the proportions assigned to these gases, it is surely sufficient that one of them (the gas from moist charcoal) is stated to contain in 100 cub. in. = $14\frac{1}{2}$ grains, no less than 9 grains of water, a proportion absolutely inconceivable; and the same objection applies, in a less degree, to the other cases. Now in 100 cubic inches of the muriatic acid gas, I found the absolute quantity of combined water to be only 1.4 gr. (Phil. Transf. 1800); and it is rendered highly probable, by the experiments of Clément and Desorme (*Ann. de Chim.* XLII. 121) that all gases contain the same quantity of water. In the instance of the gas from charcoal, Mr. C. was most probably misled, by not having suspected the presence of the carbonic oxide; and the correction is to be made as follows. One hundred cubic inches ($=14\frac{1}{2}$ grains) combined with the proper quantity of oxigen, gave 19 grains of carbonic acid, containing very nearly 4 grains of carbon; and supposing the carbon in the gas before combustion to have been in the state of carbonic oxide, it would be combined with about 9 grains of oxygen, and would constitute 13 gr. or $43\frac{1}{2}$ cub. in. of carbonic oxide.—There remains then only $1\frac{1}{2}$ grain, of the $14\frac{1}{2}$ submitted to experiment to be accounted for, which is very exactly made up by the residuary $57\frac{1}{2}$ cub. in. of hydrogen gas, taking the weight of 100 cub. in. of hidrogen to be 2.6 gr. The water contained in the gas may, I think, be set out of the question; for it must be recollected that the product of the combustion is in part aeriform; and it may be considered as a tolerable approximation to the truth, that the gas from charcoal contains, in 100 inches, 43 of carbonic oxide, the remainder being principally hydrogen gas.

Carbonic oxide does not contain combined hidrogen, &c.

With respect to the presence of hydrogen in the carbonic oxide, which has been a topic of controversy, neither the fact nor the negative can, I think, be at present with certainty affirmed. If however any hydrogen be contained in it, I should deem it an accidental and not an essential ingredient, and am of opinion that, if present at all, it exists in the state of hydrogen

gen gas; for I find that the carbonic oxide is not expanded by electrical discharges, which would assuredly happen if the carburetted hydrogen were one of its constituents, or accidentally mixed with it.

Elucidation of the theory of lamps, &c.

To return to the theory of lamps, &c. it is proved by the preceding experiments, that the substances ordinarily employed as sources of artificial light, viz. oil, tallow, and wax, afford when submitted to an increased temperature, much olefiant gas; and it has been justly observed by the editor of this Journal (4to series I. 71) "that the wick of a lamp or candle surrounded by flame is exactly in the situation of a body exposed to destructive distillation in a close vessel." In this case the series of capillary tubes composing the wick, serve perhaps precisely the same office as a tube horizontally disposed in a heated furnace, through which an inflammable liquid is transmitted. The fuel previously melted, is drawn up into these ignited capillary tubes, and there resolved into olefiant and carburetted hydrogen gases, from the combustion of which gases, and not merely of a condensible vapour, it appears to me that the illumination chiefly proceeds. Hence it is not improbable, that the proportion of olefiant gas and hydrocarburet, obtained by the distillation of any substance, will be a tolerable measure of its fitness for affording light. In distillations of this kind, however, the degree of heat is of considerable moment, for I have found that the olefiant gas may be obtained or not, at pleasure, during the decomposition of ether, alcohol, oil, &c. by varying the temperature to which the containing vessels are exposed.

The oil, &c. is decomposed in the wick,

—into olefiant and carburetted hydrogen gases, which are then burned. Whence the fitness of any fuel to give luminous flame may be known from its destructive distillation.

In the gases from coal, peat, and wood, though these substances yield no olefiant gas, the defect is compensated by an inflammable vapour diffused through them when recent, and which is even not removed by passing through a small quantity of water. Gas from coal, however, which had stood over water upwards of a month, I have found burns with considerably impaired brilliancy, though still with a far more dense and bright flame than hydrogen gas, or the gas from charcoal.

The gases from coal, &c. though they offered no olefiant gas, afford much light by an inflammable vapour when recent.

Manchester, *May* 4, 1805.

POST-

POSTSCRIPT.

Since the preceding pages were written, I have examined a fresh portion of the gas from coal, obtained by very cautious distillation, with a view to ascertain whether any olefiant gas can be procured from that substance. Of this gas five measures mixed with five of oxygenized muriatic acid, were reduced to nine; from which it should appear that the gas from coal may possibly contain $\frac{1}{10}$ of olefiant gas. The production of oil, however, was not so manifest as in other instances; and I judged it to have happened chiefly because an iridescent film was visible on the surface of the water when held between the eye and the light.

The gas from coal appears by exp. with ox. m. acid gas to contain a small portion of ol. gas.

I am enabled also, by a letter received this morning from a friend who is well acquainted with the progress of Mr. Murdoch's experiments, in answer to some queries from me, to state specifically the grounds of that gentleman's claim to the important application of coal as a source of artificial light.— This I cannot do better than by an extract from the letter.

History of Mr. Murdoch's experiments for giving light by gas from pit-coal.

"In the year 1792, at which time Mr. Murdoch resided at Redruth in Cornwall, as Boulton and Watts principal agent and manager of engines in that county, he commenced a series of experiments upon the quantity and quality of the gases contained in different substances. In the course of these, he remarked, that the gas obtained by distillation from coal, peat, wood, and other inflammable substances, burnt with great brilliancy upon being set fire to; and it occurred to him, that by confining and conducting it through tubes, it might be employed as an economical substitute for lamps and candles. The distillation was performed in iron retorts, and the gas conducted through tinned iron and copper tubes, to the distance of 70 feet. At this termination, as well as at intermediate points, the gas was set fire to, as it passed through apertures of different diameters and forms, purposely varied with a view of ascertaining which would answer best. In some, the gas issued through a number of small holes, like the head of a watering pan; in others it was thrown out in thin long sheets, and again in others in circular ones, upon the principle of Argand's lamp. Bags of leather and of varnished silk, bladders, and vessels of tinned iron were filled with the gas, which was set fire to, and carried about from room to room, with a view of ascer-

History of Mr. Murdoch's experiments for giving light by gas from pit coal.

ascertaining how far it could be made to answer the purpose of a moveable or transferable light. Trials were likewise made of the different quantities and qualities of gas produced by coals of various descriptions, such as the Swansea, Haverford-west, Newcastle, Shropshire, Staffordshire, and some kinds of Scotch coals."

"Mr. Murdoch's constant occupations prevented his giving farther attention to the subject at that time; but he again availed himself of a moment of leisure to repeat his experiments upon coal and peat, at Old Cumnock in Ayrshire in 1797; and it may be proper to notice that both these, and the former ones, were exhibited to numerous spectators, who, if necessary, can attest them. In 1798, he constructed an apparatus at Soho Foundry, which was applied during many successive nights, to the lighting of the building; when the experiments upon different apertures were repeated and extended upon a large scale. Various methods were also practised of washing and purifying the air, to get rid of the smoke and smell. These experiments were continued, with occasional interruptions, until the epoch of the peace in the spring of 1802, when the illumination of the Soho manufactory afforded an opportunity of making a public display of the new lights; and they were made to constitute a principal feature in that exhibition. I do not know exactly at what time the first trials were made, or published in France. The first notice we received of them here, was in a letter from a friend at Paris, dated the 8th of Nov. 1801, in which he desires me to inform Mr. Murdoch, that a person had lighted up his house and gardens with the gas obtained from wood and coal, and had it in contemplation to light up the city of Paris."

"After mentioning the above, I think it is proper to state also, that in the ovens constructed upon Lord Dundonald's plan, at Calcutts in Shropshire, for the purpose of saving the tar, &c. which escapes during the coaking of coal, it has been usual for a number of years past to set fire to the large current of gas as it flies off, and thus procure a bright illumination. This however was not known to Mr. Murdoch, and was never seen by him."

Experi-

II.

Experiments on the Analysis of Goulard's Extract, or the Aqua Lithargyri acetati. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

To Mr. NICHOLSON.

DEAR SIR,

I HAVE the pleasure to transmit to you some experiments on the analysis of Goulard's extract, which I hope you may think not unworthy of a place in your Journal.

I am, Sir,
Your obedient Servant,
JOHN BOSTOCK.

Liverpool,
May 5, 1805.

During the course of some experiments on the analysis of animal fluids, I was led to observe the effect of the *aqua lithargyri acetati*, or the extract of Goulard, as a coagulator of mucus, and particularly to notice the superiority of its power over that of the acetate of lead. From these circumstances I was induced to examine into the opinions that had been entertained respecting its composition, but was not able to obtain any satisfactory information. Although it is a compound so well known, and so frequently employed, it appears never to have been made the subject of chemical analysis. In Dr. Thomson's System of Chemistry it is not distinguished from the common acetate of lead;* Dr. Murray informs us, that "it is merely a solution of acetate of lead in water, with an "excess of acid;†" and Dr. Duncan, Jun. conceives, that it does not differ from a solution of the acetate of lead of the same strength.‡ We meet with nothing specific respecting its constitution in M. Fourcroy's "*Systeme*", § nor is there any light thrown upon it by his acute commentator Proust.|| In this dearth of information, I proceeded to make the following experiments.

The extract of Goulard coagulates mucilage more than the acetate of lead.

It has not been examined.

* Chem. III. 52. † Mat. Med. II. 223. ‡ Edin. Disp. p. 506
§ VIII. 203. || Journ. Phys. LVI. 207.

To

Experiments on its composition. The common acetate of lead was dissolved in water.

1. To 200 grains of distilled water were added 60 grains of acetate of lead, in its usual crystalline state; the fluid was kept for about an hour at the boiling temperature, and was afterwards filtered. The residue, when dried, did not amount to more than 2 grains; this was boiled in a fresh quantity of water, when about half of it was dissolved, but one grain still remained not acted upon by the water. It appeared, therefore, that a saturated solution of the acetate of lead was formed; it was transparent and colourless; it slightly reddened paper stained with litmus.

This was precipitated by carbonate of potash. It gave eleven parts carbonate of lead.

2. A solution of the carbonate of potash was prepared, in the proportion of 11.25 grains of potash to 100 grains of water. To 40 grains of the solution of the acetate of lead from No. 1, a quantity of this solution of potash was gradually added; a copious precipitate of the carbonate of lead was produced; after the addition of 60 grains of the alcaline solution there was no farther precipitation, and the fluid slightly affected a paper soaked in the infusion of the mallow flower. The precipitate was carefully collected, and being dried by a gentle heat, afforded eight grains of carbonate of lead.

Goulard's extract gave eleven parts carbonate of lead.

3. Forty grains of *aqua lithargyri acetati* were treated in the same manner with the carbonate of potash; the precipitate formed appeared more copious than in the former experiment, and after the addition of 40 grains only of the alkali, no farther effect seemed to be produced; the fluid affected the mallow-paper in the same degree as in the former experiment. The precipitate, being collected and dried, weighed 11 grains.

It did not redden litmus paper. Blue precipitate with litmus infusion.

4. The *aqua lithargyri acetati* did not in any degree redden litmus paper; a few drops of it were added to half an ounce of the infusion of litmus; a precipitate of a beautiful light blue was immediately formed, while the fluid was left transparent and nearly colourless.

The solution of acetate gave 4 gr. brittle clear residue by evap.

5. Twenty grains of the solution of acetate of lead, No. 1, were slowly evaporated; the fluid became extremely viscid, and at length, in some degree, brittle and transparent, and assumed the appearance of dried gum. It weighed about four grains.

Aqua lith. acet. gave 5 gr. white opake residue.

6. Twenty grains of the *aqua lithargyri acetati* were evaporated in the same manner; it became white and opake, and when the process was completed, it exhibited the appearance of a number of scales of a pearl colour. It weighed a little more than five grains.

7. A

7. A solution of gum Arabic was formed, in the proportion of one part of gum to 100 parts of water. One grain of this solution was added to 39 grains of water, so that the gum only constituted $\frac{1}{4000}$ part of the solution; a single grain of Goulard dropped into it produced a perceptible opacity. Gum precipitated by Goulard's extract.

8. Twenty grains of the saturated solution of the acetate of lead, had one grain of the solution of gum added; the effect was barely visible, certainly less than in the former experiment. —but scarcely by acetate of lead.

I am far from considering these experiments as sufficient to afford a complete investigation of the subject; but I think they may enable us to make some advances towards the truth.

The 40 grains of the solution No. 1. contain 11.6 grains of acetate of lead; by the addition of the alcali, this was converted into eight grains of the carbonate of lead. These eight grains of carbonate consist of 6.72 grains of the yellow oxide, and 1.28 grains of carbonic acid.* The 6.72 grains of yellow oxide consist of 6.12 grains of pure lead, and .6 grains of oxigen, † so that the 40 grains of the saturated solution contain a little more than 6 grains of pure lead. Forty grains acetate contain six grains metallic lead.

By employing the same reasoning to the analogous experiment with Goulard, we may conclude, that the 11 grains of carbonate produced in this case, consist of 9.24 grains of the yellow oxide of lead, and 1.76 grains of carbonic acid; the 9.24 grains of oxide will be composed of 8.4 grains of pure lead, and .84 grains of oxigen, so that the 40 grains of the *aqua lithargyri acetati* contain nearly $8\frac{1}{2}$ grains of pure lead. Forty grains Goulard contain $8\frac{1}{2}$ of lead.

We shall not find it so easy to ascertain precisely the quantity of acetic acid which enters into the composition of the acetate of lead, and the *aqua lithargyri acetati* respectively; but if we trust to the experiments of M. Thenard ‡, we must conclude that 11.6 grains of acetate of lead, contain about three grains of the acetic acid. The quantity of acid in the *aq. lith. acet.* is less than that in the acetate of lead, in the proportion of 40 to 60; therefore the 40 grains of *aq. lith. acet.* will only contain two grains of acid. Deduction of the respective quantities of acid.

Hence it follows, that 40 grains of the solution of acetate of lead consist of, Component parts of each.

* Thomson's Chem. III. 50. † Proust, Journ. de Phys. LVI. 206. ‡ Nich. Journ. VI. 223.

Lead

	Grs.			Grs.
Lead - -	6.12	The ſame quantity of Goulard will conſiſt of - -	Lead	8.4
Acetic Acid	3.		Acid	2.
Oxygene	.6		Oxygene	.84
Water	30.28		Water	28.76
	40.00			40.00

converting theſe proportions into quantities of 100 grains each they will be as follows,

	Sol. acet. lead.	Aq. lith. acet.
Lead - - -	15.3	21.
Acetic acid -	7.5	5.
Oxygene -	1.5	2.1
Water - -	75.7	71.9
	100.0	100.0

The experiments ſhew that Goulard and the acetate are different ſalts.

From this ſtatement it appears, that in the *aq. lith. acet.* the oxide of lead and the acid exiſt to each other in the proportion of 23 to 5, or of 100 to 21.74, and we find that M. Thenard has deſcribed the perfect acetate of lead, as a ſalt in which the oxide and the acid exiſt in the proportion of 100 to 21.79. So near a coincidence between theſe two proportions can ſcarcely be regarded as the mere effect of accident; but muſt rather be conſidered as a proof that the ſubſtances operated upon were nearly, if not altogether, identical. Admitting this to be the caſe, we muſt conclude that the *aqua lithargyri acetati* is a ſaturated ſolution of the proper acetate of lead, that it is an eſſentially different ſalt from the ſuper-acetate of lead, and that it is not, as has been imagined, an accidental compound, but an exactly neutralized ſalt, the conſtituents of which exiſt in a conſtant ratio to each other.

The former being at ſaturation,

—and the latter a ſuper-acetate.

The neutral compound is moſt eaſily decompoſed,

—and is therefore a better teſt of mucus.

It happens in this, as in other inſtances, that the ingredients compoſing the completely ſaturated compound, poſſeſs a weaker affinity for each other than when they exiſt in a different proportion. To this circumſtance muſt be attributed the ſuperior delicacy which Goulard poſſeſſes, as a teſt of animal and vegetable mucus, over the ſuper-acetate of lead, or the common *ceruſſa acetata*. The *aqua lithargyri acetati* is ſpeedily decompoſed by the action of the atmoſphere, in conſequence of the oxide of lead which enters into its compoſition having a ſtronger affinity for carbonic than for acetic acid; this effect takes place in a leſs degree, in a ſaturated ſolution of ſuper-acetate of lead.

From

From the firft experiment we learn, that the fuper-acetate of lead is more foluble in water than is generally imagined; Dr. Thomfon obferves, that it is diffolved only fparingly *; yet we find that 100 parts of water retain in folution 27 parts of the falt. Super-acetate of lead more foluble than generally fuppofed.

III.

A concife View of the Theory of Refpiration. By W. Brande', *Efq.* *(From the Author.)*

THE term refpiration implies the reception of atmofpheric air into the lungs, and its fubfequent emiffion, after having produced changes in the blood neceffary to the continuance of life †. Refpiration,

No other gafeous body being capable of producing thefe changes, it was natural to fuppofe, that until we became acquainted with the component parts of the atmofphere, very little of the true nature of refpiration could be underftood. could not be explained till the atmofphere was analyfed.

The firft great ftep towards the analyfis of the air was made by Dr. Prieftley, who in the year 1774 difcovered oxigen gas, called by him dephlogifticated air. But we are indebted to Lavoifier for the moft accurate inveftigation on this fubject; who from many experiments, which it is not neceffary here to relate, concluded that atmofpheric air was compofed of oxigen and azot, in the proportion of about 27 parts of the former to 73 of the latter. The air alfo contains a fmall quantity of carbonic acid, and a confiderable quantity of water (fubject however to much variation) is always fufpended by it. Difcoveries of Prieftley, and Lavoifier.

Some of the gafes are totally unrefpirable, that is to fay, incapable of being taken into the lungs; for whenever this is attempted, a fpafmodic affection of the epiglottis takes place, which by clofing on the larynx, fhuts up all communication with the organs of refpiration. To this clafs belong all thofe Gas which cannot be refpired, or admitted into the lungs.

* Thomfon's Chemiftry, III. 53.

† Refpiration has been divided into, 1. Infpiration, or the ingrefs of air into the cells of the lungs, caufed by the enlargement of the cavity of the cheft; 2. Into expiration, or the egrefs of air from the lungs, caufed by the contraction of the cheft.

gafeous

Carbonic acid. Its effects; as described by Pilatre de Rozier.

gaseous bodies possessed of acid properties. The effects of carbonic acid are described as follows by Pilatre de Rozier:— He went into a brewer's tub which was full of carbonic acid gas; he at first felt a slight heat throughout his whole body, which produced a gentle perspiration; an itching sensation frequently obliged him to close his eyes, and on attempting to breathe, he was prevented by a very violent sense of suffocation. He wished to get out of the tub, but being unable to find the ladder, the necessity of breathing increased, he was seized with a violent giddiness, and felt a tingling sensation in his ears. He at length contrived to extricate himself, and although he then experienced no difficulty in breathing, he was unable to distinguish the objects around him; his hearing was also much impaired. On repeating the experiment he found, that as long as he remained without attempting to breathe, he could readily move or even speak, but whenever he tried inspiration, a violent sense of suffocation came on.

Gases which can be admitted into the lungs.

But there are certain gaseous bodies which may be drawn into the lungs, meeting with no opposition from the organs of respiration. Dr. Thomson has divided these into four classes. The first set, he observes, occasion immediate death, but produce no visible change in the blood; they occasion the animal's death, merely by depriving him of air, in the same manner as were he immersed in water: the only gases belonging to this class are hidrogen and azot. The second set occasion immediate death also, but at the same time produce certain alterations in the blood; and therefore kill, not only by depriving the animal of air, but by certain specific properties: the gases belonging to this class are, carburetted hydrogen, sulphuretted hydrogen, carbonic oxide, and perhaps also nitrous gas. The third set of gases may be breathed for some time without injury, but death ensues at last, provided their action be long enough continued: to this class belong the nitrous oxide and oxigen gas. The fourth set may be breathed any length of time without injury: the only gaseous body belonging to this class, is the air of the atmosphere, that compound of oxigen and azot every where surrounding the globe.

Hydrogen and azot kill by mere suffocation.

Gases which kill by a speedy positive action.

Nitrous oxide and oxigen kill by a slower action.

Atmospheric air maintains life.

This compound fluid loses its oxigen by that process, and carbonic acid gas is produced.

After an animal has breathed a certain quantity of air for a given time, it becomes totally unfit for respiration; and if the air thus respired be chemically examined, we shall find that the oxigen is greatly diminished, and that a considerable quantity of carbonic acid gas has been produced.

It

It appears from a number of experiments made by Dr. Hales, Dr. Menzies, and Mr. Davy, that the number of respirations made in a given time, as well as the quantity of air taken into the lungs, are liable to considerable variations in different people. Some have calculated the number of respirations at 14 only in a minute, others at 20; Mr. Davy informs us that he makes 26 or 27 in a minute; but having frequently endeavoured to count the respirations made by different people, in a given time, and without their knowledge, I have found them vary from 18 to 26 in a minute, most commonly, however, 20 or 21; and 21 in a minute make 30,240 in 24 hours.

The number of respirations made in a minute vary in different persons from 18 to 26; but are commonly about 20.

The quantity of air taken in at each respiration, must be in proportion to the size of the person and the capacity of his lungs. About 41 cubic inches of air are taken in at every natural inspiration.

Each respiration being about 41 cubic inches of air.

We now come to consider the changes which are produced, both in the air and blood, by respiration. 1. On the changes effected in the air. Dr. Priestley, M. Lavoisier, and Mr. Davy, have furnished us with many interesting and instructive experiments on this subject. The changes are, 1. That a portion of the air disappears; 2. That the air expired differs from that first taken into the lungs, in containing carbonic acid, and water in the state of vapour. Dr. Menzies has shewn, that $\frac{1}{20}$th of the air inspired disappears in the lungs, and the experiments of Lavoisier, which were made with much precision, differ but little from the above statement. I never knew the quantity of air which disappears to be less than $\frac{1}{19}$th part of the whole taken into the lungs; this may however be liable to variation in different people.

Phenomena of respiration.

Some air disappears; about one-nineteenth; and the expired air is altered.

It has hitherto been supposed that the portion of air which disappears, consists of the oxigen only: Mr. Davy has, however, given some very strong reasons for supposing that part of the azote likewise disappears during respiration. He supposes that the average quantity of air which is absorbed at every respiration, amounts to 1.4 cubic inches, of which 0.2 are azot and 1.2 oxigen.

The absorbed air was thought to be oxigen only; but appears to be in part azot.

Lime-water detects carbonic acid gas in the air emitted from the lungs, and the quantity of this gas may easily be estimated, by receiving the air expired into a graduated glass jar, standing over mercury; a little caustic soda being introduced, the absorption which takes place denotes the quantity of

The emitted air contains carbonic acid;

of carbonic acid. Lavoisier has estimated the quantity of this gas thrown out from the lungs in 24 hours, at about 15.5 ounces troy. Mr. Davy makes the quantity thrown out in the same time, amount to 37 ounces, which is about 1 cubic inch at every expiration *. The quantity however varies from 0.5 cubic inch to 1.5 at different times in the same person, so that this accounts for the great variation in the above-mentioned experiments. Moreover the proportion varies, in the same individual, during the 24 hours; for I have found the quantity of carbonic acid gas emitted from my own lungs, to be rather less in the morning than towards the evening; but this also varies in different people.

about one cubic inch each expiration.

But this varies.

But watery vapour is also emitted in respiration, the greatest part of which is probably given off by the exhalent arteries, which are so copiously dispersed on the surface of the lungs. A part is also emitted from the blood in the pulmonary vessels. The estimation of its quantity is attended with some difficulty; according to Dr. Hales it amounts in a day to 20 ounces: this is however but of little consequence, for it is liable to much alteration.

Aqueous vapour also emitted;

in quantity variable.

More important changes however than those just mentioned are produced by respiration, namely, the alterations produced in the blood; which fluid, returning from every part of the body by the veins, is poured into the heart; from whence, being propelled through the lungs, it is brought into contact with the air, undergoing certain changes which render it fit for the nourishment and support of the body. These changes, which are of a very complicated nature, have engaged the attention of several learned and ingenious philosophers. Dr. Thomson has enumerated them as follows: 1. The blood absorbs air. 2. It acquires a florid red colour, and the chyle disappears. 3. It emits carbonic acid, and perhaps carbon. 4. It emits water, and perhaps hydrogen. Dr. Priestley, M. Lavoisier, and Lagrange, have each adopted a different theory, by which they endeavour to explain and account for these changes produced by respiration: they are all however liable to considerable objections, and rest merely on the supposition that the oxigen is alone absorbed. Now Mr. Davy has shewn, that at least a portion of the azot disappears in the

The blood undergoes important changes during respiration.

It absorbs air; becomes florid red; emits carbonic acid; and water; and perhaps hydrogen.

Various theories.

* Davy's Researches, page 433.

lungs:

lungs: he has even rendered it very probable that the air is not decomposed, but that it is absorbed unaltered by the blood; that it is decomposed during circulation; and that the useless portion of azot is again given out. The following facts are in support of this opinion: "When the gaseous oxide of azot is respired, its quantity is diminished, carbonic acid gas is evolved as usual, and a quantity of azot makes its appearance. Now as this azot did not exist separately, it must have been produced by the decomposition of the gaseous oxide of azot; but its quantity being much less than the azot contained in the oxide of azot which had disappeared, it follows that a part of this last gas had been absorbed unaltered; and if a part, why not the whole? In that case the azotic gas must have been separated from the blood by the subsequent decomposition of the oxide of azot absorbed *." Atmospheric air is composed of exactly the same ingredients as the oxide of azot, merely in different proportions, and in a state of less intimate chemical combination. It is moreover natural to ask, that if oxigen were alone absorbed by the blood, why should it not answer the same purposes as air? It is well known that this gas cannot be respired for a length of time without producing fatal consequences; but even when it is respired, the quantity (of oxigen) which disappears is much smaller than when a like quantity of atmospheric air is breathed for the same time. Mr. Davy has given the following experiment in proof of this fact: He breathed 182 cubic inches of oxigen gas for half a minute, 11.4 c. inches disappeared; whereas when the experiment was repeated under the same circumstances with atmospheric air, the quantity absorbed amounted to 15.6 cubic inches.

Mr. Davy apprehends that azot is absorbed with the oxigen, &c.

Facts from the respiration of oxide of azot.

Oxigen alone is not proper for respiration:

Much less is absorbed than when a like quantity of atmospheric air is breathed.

It was first observed by Lower, that the colour of venous blood, which is dark reddish purple, was converted into the florid scarlet colour of arterial blood, in its passage through the lungs. The phenomena of respiration, however, still remained unexplained, until Dr. Priestley published his experiments on the changes produced in venous and arterial blood when put in contact with certain gaseous bodies. "† He observes, that having introduced pieces of the crassamentum of

Lower first observed the change of colour in venous blood by respiration.

Priestley shewed the cause; viz. the absorption of oxigen.

* Thomson's System of Chemistry, Vol. IV. page 719.
† Priestley on Air, Vol. III. page 71.

coagulated

coagulated ſheep's blood into dephlogiſticated air (oxigen gas); the blackeſt parts aſſumed a florid red colour, and that more readily than they would have done if common air only had been made uſe of: Whereas the brighteſt red blood became preſently black in any kind of air unfit for reſpiration, as in fixed air, &c.; and after having become black in phlogiſticated air (azot), it regained its red colour on being brought into contact with common air, the ſame blood becoming alternately black and ſcarlet, by being transferred from phlogiſticated into dephlogiſticated air, and *vice verſâ*."

Theſe then may be regarded as the experiments which gave origin to all ſubſequent enquiries.

Deſcription of the proceſs of nutrition.

The food which is taken into the body is converted into chyle and excrement *; the former of which is abſorbed by a ſet of veſſels termed lacteals, which convey their fluid into the thoracic duct. The term lymph has been applied to that fluid which lubricates the ſurfaces of all the circumſcribed cavities of the body: This fluid is abſorbed by a ſet of veſſels termed lymphatics, which of courſe originate in every part of the body; they likewiſe terminate in the thoracic duct, which therefore is the great reſervoir of the abſorbent ſyſtem; it receives the chyle and lymph, and conveys them to the blood; they are here decompoſed, and converted into new ſubſtances neceſſary to the ſupport of the body. Now the coagulable lymph, or fibrina, appears to be the moſt eſſential part of the blood, for it is employed to ſupply the waſte of the muſcles, &c. and Dr. Thomſon has accounted for its formation in the following mannner †: "It follows," ſays he, "from the experiments of Fourcroy, that fibrina contains more azot and leſs hydrogen and carbon than any of the ingredients of the blood, and conſequently alſo than any of the ingredients of the chyle. In what manner the chyle, or a part of it, is converted into fibrina, it is impoſſible to ſay: We are not ſuffi-

Formation of fibrina.

* The food, on being received into the ſtomach, is converted into a pulpy ſubſtance termed chyme. This alteration is effected by a peculiar fluid called *gaſtric juice*, which is ſecreted by the internal coats of the ſtomach. The chyme thus formed is propelled into the duodenum, where it meets with the bile, which converts it into a fluid much reſembling milk, termed chyle, and into excrement.

† Thomſon's Chemiſtry, 2d Edit. Vol. IV. page 725.

ciently

ciently acquainted with the subject to be able to explain the process. But we can see at least, that carbon and hydrogen must be abstracted from that part of the chyle which is to be converted into fibrina, and we know that these substances are actually thrown out in respiration. We may conclude then that *one* use of the air absorbed is to abstract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in such a manner that the remainder becomes fibrina: Therefore one end of respiration is to form fibrina.

Use of respiration in that connexion.

It appears then, from the above-mentioned facts, that the perfection of the blood is almost totally dependent on respiration; whenever therefore this function is suspended but for a very short time, death is the consequence.

Life cannot subsist without it.

It is well known that all the more perfect animals possess a temperature considerably higher than the surrounding atmosphere: the cause however of this increased temperature, remained unexplained for a considerable time. At length Dr. Black's theory of latent heat became known, when several attempts were made to explain the cause of the increase of temperature, or standard heat of the body, but none of them were satisfactory. Dr. Thomson has however given us the following ingenious theory: As the air is absorbed unaltered by the blood, it is evident that it will give out the greatest portion of its caloric during circulation; that portion therefore which is emitted at the instant that the air combines with the blood, is united to the carbonic acid, converting it into the state of gas, and the water into vapour. It appears moreover, that the heat of the blood is somewhat raised during circulation; for Mr. John Hunter found that the blood in the heart was a degree higher than in any other part of the body.

The elevated temperature of the more perfect animals is caused by the combination and condensation of air.

From the facts which have now been alluded to, it appears that the following changes are produced by respiration: The blood is propelled, by the contraction of the heart, into the pulmonary artery, which, by its numerous ramifications, conveys the blood into the small branches of the air-cells of the lungs, which are of so fine a texture as to admit the absorption of a portion of air. The blood having undergone this alteration, is returned into the heart by the pulmonary veins, from whence it is circulated over the whole body. During the circulation, the air which has been absorbed undergoes a gradual decomposition; carbonic acid and water are formed, which, together

Recapitulation.

together with a portion of azot, are returned by the veins, and thrown out as the blood passes through the lungs. A fresh portion of air is at the same time absorbed, and the above changes repeated.

These then are the effects of respiration, as far as we are at present acquainted with them; but this important branch of physiology still remains in considerable obscurity.

Arlington Street, WILLIAM BRANDE.
April 29, 1805.

IV.

Instruction on the Processes discovered by M. Bralle, *of Amiens, for watering Hemp in Two Hours Time, in all Seasons, without injuring its Quality* *. *Published by Order of the Minister of the Interior of France.*

Experiments on hemp, by order of the French government.

IN the month of Fructidor in the year XI. (September 1803.) the government called to Paris M. Bralle, of Amiens, the inventor of new processes for watering hemp. This discovery, which is interesting to agriculture, manufactures, commerce, and the marine, had engaged its attention; orders were given to make the experiments requisite to ascertain its value.

Every thing which could elucidate the principles and practice of M. Bralle's processes, which could prove and insure their success, was put in practice. Numerous and varied trials were made in the presence of M. M. Monge and Berthollet, senators, and Teissier, member of the Institute. M. Molard, administrator of the conservatory of arts and manufactures, directed these trials, and carefully pursued them for six months. The results were equal to the expectations that had been formed.

From the account rendered to his imperial Majesty, it was judged that the knowledge of a more expeditious method of watering hemp, than those employed at present, which is practicable at all seasons, and is in no respect injurious to health, by means of which a greater produce can be obtained from an equal quantity of the materials; and, which must extend

* From Bibliotheque Physico Economique, Brumaire, An. XIII.

and

and multiply the culture of an extremely valuable plant, could not be too extensively published. In conformity to this desire, we shall briefly describe M. Bralle's processes, relate the experiments which have been made, and offer some observations on the utility and advantages which are promised by this new discovery.

§ I.

The Processes of M. Bralle.

The means used by M. Bralle for watering hemp, are

M. Bralle's process. The hemp is steeped in hot water with soap.

1st. Water is heated in a vessel to the temperature of from 72° to 75° of Reaumur's thermometer; (200° fahr.)

2nd. A quantity of green soap (*savon verd*) is added proportional to the quantity of the hemp to be steeped.

3rd. The hemp is then immersed so that it shall be covered by the fluid, after which the vessel is closed, and the fire put out.

4th. The hemp is left in this state of maceration for two hours, and then taken out.

Proportions of the articles.

The weight of soap required for a complete steeping, is to that of hemp-stalks as 1 to 48; and the weight of the hemp to that of the water as 48 to 650.

The process may be repeated with other hemp in the same water.

Several steepings may be made one after the other. It is sufficient, before each new immersion, to add a quantity of soap water to replace what was absorbed by the preceding, and to raise the temperature of the bath to the above degree. The same water may be thus employed for fifteen successive days.

When the bundles are taken out of the steeping vessel, they are covered with a layer of straw, that they may cool gradually, without losing their humidity.

Breaking the stalk,

Next day they are spread on a floor, pushing the bands towards the top of the stems, and a roller of stone or wood, loaded with a weight, is passed several times over them, to crush them, and dispose the tow to be easily separated from the reed, which is effected by beating. Whether the hemp be wet or dry, it peels completely in either state.

beating.

After having tied the handfulls of the tow peeled off while wet, at the top, they are spread on the grass, turned, and, after five or six days, carried to the warehouse.

The

The handfulls of steeped and crushed hemp which are intended to be beaten and stripped dry must also be exposed on the grass; this operation being absolutely necessary to whiten the tow, and facilitate the separation of the reed.

§ II.

Recapitulation of the Experiments.

The experiments were varied to ascertain the requisite

By means of a portable steeping vessel, different quantities of hemp were steeped, the temperature of the soapy liquor was varied at pleasure, and the state of the hemp was observed during the course of each operation, of which the duration was more or less prolonged, in order to ascertain;

Temperature,

1st. The temperature which the soapy liquor ought to have before the immersion of the hemp;

time, and

2nd. The time necessary for a complete steeping, at a determinate temperature;

proportion of soap.

3rd. The quantity of soap absolutely necessary for a given weight of hemp-stalks, weighed before the immersion, &c.

Results or general observations on the process.

From a great number of experiments made in the months of January, February, and March last, it was found,

1st. That water containing the quantity of green soap directed by M. Bralle, for a given weight of hemp, effects the steeping completely;

2nd. That the steeping is so much the more speedy as the temperature of the fluid is nearer to ebullition, at the time of the immersion of the hemp;

3rd. That if the hemp be kept more than two hours in the steeping vessel, the time prescribed by M. Bralle for obtaining a complete watering, the tow separates equally well from the reed, but it acquires a deeper colour, and loses part of its strength;

4th. That if the hemp be immersed in a cold soapy liquor, and heat be then applied, the steeping is not accomplished so perfectly, whatever degree of temperature may be given to the liquor, and however long the immersion may be continued *;

5th. That the bundles of hemp immersed and kept vertically in the vessel, are steeped more uniformly than if they were laid horizontally; and this position also facilitates the manipulation.

* Probably the fluid between the fibres is not heated, because its conducting power is bad, and it is prevented from circulating. N.

§ III.

§ III.

Observations on the Utility and Advantages of the New Discovery.

Two methods only of steeping hemp are generally practised. The first consists in spreading the plant on the grass, and turning it two or three times a week, until the air, the light, the dews, or the rains, have disposed the tow to separate easily from the reed. The result is obtained in a longer or shorter time, according to the weather and the state of the air; and frequently, in certain countries, the operation is not finished in less than forty days.

Description of the methods of steeping hemp, as heretofore practised.
Exposure on the grass.

The second consists in immersing the bundles of hemp in rivers, brooks, ditches, or pools, and keeping them there for eight, fifteen, twenty, or even thirty days, according to the degree of the heat of the water, or of the atmosphere.

2. Steeping.

The maceration effected by both these processes is frequently incomplete, and always unequal. By following the first, the cultivator is liable to have his crop dispersed by the winds, or injured by long rains: if he adopts the second, he runs the risk of losing a part of it by the overflowing of the rivers, or of its being covered with mud. The first method in particular, is liable to the serious inconvenience of depriving the national marine of part of the hemp produced by our territory: it is known that the tow produced from the hemp which has been exposed on the grass is not used by the government.

These processes very defective.

The steeping of hemp according to M. Bralle's process, requires only a copper cylindrical vessel, placed on a small furnace of bricks.

Superiority of the new process.

A steeping vessel of this kind, containing 240 litres of water, (52 ale gallons) is sufficient to steep 18 kilogrammes of hemp-stalks, (about 40lb.) and as the operation is completed in two hours, 100 kilogrammes (221lb.) may be easily steeped in a day.

This method appears to deserve the preference over the former ones, on many accounts.

1st. The steeping is practicable all the year, except during very hard frosts, when it is difficult to dry the hemp. But when it is to be peeled wet, the cold is no longer an obstacle; it is then only necessary to take proper precautions for preventing the tow from freezing in its humid state.

It is practicable all the year.

 2nd. The

saves time,

2nd. The time of steeping being only two hours, affords a saving of time of great value to the cultivator, particularly during the season of harvest.

and is not injurious to health.

3rd. The workman has no cause to fear any injury of his health: it is sufficient to keep up a current of air while the bundles are plunged into and taken out of the steeping vessel; the handfulls of stalks or tow, which are afterwards exposed on the grass, do not emit any bad smell, or vitiate the purity of the air, whatever may be the quantity of hemp dried at once in the same place.

Every one knows, that when the bundles of hemp steeped in water in the old method, are taken out and washed, they emit an infectious odour which becomes insupportable during the heats, and to which serious disorders are ascribed. The valley of the department of the Somme, and many others in which hemp is steeped, afford too convincing proofs. The waters are rendered unfit for the use of cattle, and the fish contained in them are frequently destroyed.

Apparatus on a larger scale.

To accelerate the operation of steeping by the new process, in countries where there is an extensive culture of it, instead of the portable steeping vessel which was made use of in the experiments, the following apparatus may be adopted, consisting of a boiler and four wooden tubs, serving for steeping vessels.

After having heated the soapy matter to ebullition, it is suffered to flow through a cock, into two of these tubs filled with bundles of hemp, and closed by a cover: while the steeping is going on in the two first tubs, the necessary quantity of liquor is heated, to be conveyed into the other two, which are also filled with bundles of hemp, and closed with lids.

By means of this very simple apparatus, a considerable quantity of hemp may be steeped in a day without interruption.

Comparative expence of the two processes.

4th. The expence of steeping in water, compared with that required by the method of M. Bralle, is nearly the same, when the small steeping vessel is made use of; but if a cauldron, rather large, and the steeping tubs which have been mentioned are employed, the cost will be diminished more than a half.

In fact, the expence of the first includes the conveyance of the hemp to be steeped, the time employed in forming the bundles of hemp into a sort of rafts, that they may be sunk by

by loading them with stones, turf, clods of earth, and even mud; in fixing and securing these rafts by driving in stakes; a tedious work, and the more troublesome, because 10 kilogrammes of hemp-stalks cannot be immersed without a weight of 15 or 20 kilogrammes, and, after the steeping, all this mass must be removed, to take the bundles out of the water, and wash them.

The cost of the new process consists principally in the price of the solvent made use of, which amounts to about eight centimes for a kilogramme of tow. To this should be added the price of combustible necessary for heating the liquor, if this combustible was not afforded by the reeds of the bundles, whether they are peeled wet or dry.

At an equal expence, the new process is still preferable to the old, because, from what has been said, it renders the manipulation more expeditious and more easy.

5th. Eight kilogrammes of hemp-stalks steeped by the new process, commonly produce two kilogrammes of pure tow, by peeling when wet; whereas hemp steeped in water by the old process, and beaten, does not yield more from eight kilogrammes than one and a half. Comparative produce.

The dry peeling of hemp steeped in the old way does not produce the same quantity as that which is peeled when wet: the breaking of the reed in many places occasions a greater loss of tow.

The hemp being washed, beaten and combed in the old method, a kilogramme of long tow is obtained from four kilogrammes of the rough tow; the remainder is short stuff, hards and dust.

The same quantity of hemp, manipulated in the new way, yields two kilogrammes of long tow, one kilogramme of second tow, and about a kilogramme of short stuff and hards.

Thus from eight kilogrammes of hemp-stalks, two kilogrammes are obtained in rough tow by the new process, and from this quantity is obtained one killogramme of the first tow, which does not exist in any known manipulation.

6th. The inhabitants of the banks of rivers and of the valleys, are almost the only persons who cultivate hemp: they owe this privilege to the vicinity of the waters, and the humidity of the soil. By the new process the culture of hemp will be extended to all places, and procure a new and very Extension of the culture of hemp.

very advantageous occupation to the inhabitants of the plains, the land of which is much more vegetative than that of the marshes.

It is an error to suppose that hemp cannot grow to a great height in the plains; it is a fact, that it rises to the height of two yards, in land which has been well ploughed and manured, when mild rains have promoted germinanation and growth.

It is equally a fact, that there is every where a sufficient quantity of spring or cistern water to steep the hemp by the new process: if droughts should supervene, which besides are only accidental, the steeping may be deferred.

It will, therefore, be possible to cultivate hemp in the plains, and in low lands, which are always rich and fertile, though frequently without springs of water, and to augment not only the mass of our products, but also our riches of this description, since one acre of good hemp yields as much profit as two acres of wheat.

Summary of the advantages arising from this process.

Such are the effects which may be expected from M. Bralle's new method of steeping hemp. It is, as was observed at the commencement of this instruction, more expeditious than those hitherto employed; it perfectly completes the steeping; it may be used at all seasons; it does not affect the purity of the air; from an equal quantity of materials, it procures a more abundant produce; and lastly, it is well calculated to extend the cultivation of the plant itself. The enlightened lovers of agriculture, and well-informed proprietors, who live upon and cultivate their own estates, without being slaves to the customary practices, will adopt it, and secure its advantages, by repeating the experiments which have ascertained its merit, and also by making trials on a more extensive scale than those which took place in the conservatory of arts and manufactures. Their example will be followed, the process of M. Bralle will be extended, and we shall see portable steeping vessels, similar to those used by M. Molard, multiplied; a cheap apparatus which requires very little repairs, and by means of which the hemp grown through the extent of one or of several communes may be steeped even in the field on which it grew.

V.

V.

Description of a Portable Steam Engine. By Mr. Matthew Murray.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of handing you the description of a portable steam engine of my construction, which you will have the goodness to insert in your Philosophical Journal. I will just observe it is reduced to the fewest parts that practical utility will admit, which must necessarily render it of great advantage; as the simplicity of its parts make it nearly impossible to be out of order with a very moderate degree of management. The following description and reference to the plate will explain the nature of this engine.

I am, Sir,

Your much obliged humble servant,

MATTHEW MURRAY.

Leeds, May 7*th*, 1805.

Description of a Portable Steam Engine.——Plate VII.

Description of a portable steam engine.

A A Represents the ground or floor on which the engine stands.

B. Section of a recess made in the ground for the beam O to work in.

C. Iron cistern resting upon the ground or floor covering the recess for the beam.

D An opening in the floor to admit a boy to oil the centers of the beam.

E A double steam cylinder, having an upright pipe in the intermediate space, which effects a communication between the top and bottom and the valve box G.

F A steam pipe that communicates with the boiler through which all the steam passes and surrounds the inner cylinder in its way to the valve box, prior to its application against the piston.

G The valve box fixed upon a projection from the cylinder bottom, having an opening or connection with the interval between the two cylinders. In this opening is a regulating valve

Description of a portable steam engine.

valve for adjusting the quantity of steam (that acts against the piston) in its passage through the valve box. There are also three other openings in the bottom of this valve box, one of which connects with the top of the cylinder by the pipe in the intermediate space, the second with the bottom, and the third with the eduction pipe that leads to the condensor. Two of these openings are alternately connected together by a slide valve,* while the third is left open for the admission of steam to the piston, this valve changes its position at the end of each stroke of the piston, and performs all the purposes of the most complicated machine.

H The air-pump connected with a condenser at the bottom of the eduction pipe.

I. The fly wheel fixed upon an axis which receives its motion from a crank connected with the beam by the rod K.

L L. Two rods for connecting the motion of the piston to the beam, these rods move perpendicularly by a motion which could not be conveniently shewn in this view without rendering it confused.

M A spherical triangle turn'd by the crank for moving the slide valve by the horizontal rod N that connects them together. This motion has the advantage of preventing the engine from ever turning the contrary way round from that which it is wanted to go, and prevents the noise that is usually heard in engines.

O The beam attached to the bottom of the cistern C by means of the hanging carriages P.

Q A rest or fixture in a wall for the end of the fly wheel shaft; this will vary according to the situation where the engine is to be fixed, or it may be supported by a metal standard.

R. Index to the injection cock that admits water to the condenser. *Note*, The cistern is to be kept nearly full of water during the time the engine is at work.

The cylinder G and valve box E must be surrounded on all sides by a case (not shewn in this view) the space between filled with charcoal to prevent the transmission of heat, which if effectually done will work with the least possible quantity of coals, as it combines the advantages of every other engine hitherto known. By detaching the air-pump and condenser

* For these valves I took out a patent in 1802.

(which

Deſcription of a portable ſteam engine.

(which may be done in half an hour) and where water cannot be had for condenſation, this engine may be worked by the preſſure of ſtrong ſteam alone, as the internal cylinder is kept as hot as the ſteam in the boiler.* This dangerous plan never ought to be reſorted to but in caſes of neceſſity, as it is no ſaving of coals, and as there can be no certain rule when to diſcontinue the uſe of the boiler, the weakneſs of which is not prevented by putting the fire in a tube in the inſide of it. This engine requires no framing nor mill-wright work in the fixing, but merely bolting down to the floor it ſtands upon. It takes up very little room, and all its parts are within reach, without the neceſſity of upper floors or ſtages, which would be the caſe if the beam was above; but by being fixed below and alone, it has no tendency to move from its ſituation.

VI.

Letter from Mr. J. C. HORNBLOWER, *Engineer, on the Meaſure of Force by Horſe Powers.*

To Mr. NICHOLSON.

DEAR SIR,

Uncertainty of what is meant by the power of a horſe.

I AM induced to trouble you on account of the preſent unſettled ſtate of things reſpecting what is uſually called *the power of a horſe*. I do not know why a matter of this ſort ſhould remain ſo diſregarded, eſpecially as it has ſo long become one of our data, comprehending the unities of weight, ſpace and time, by which we are to be underſtood in our communications on the ſubject, and by which we are to aſcertain the pre-

* Many engines are at preſent worked in London and elſewhere by the mere force of ſteam, without condenſation, under Trevithick's patent. The force is from 45 to 60 lb. on the round inch; a preſſure equal to about 25 fathoms of water at the moſt. Various aſſertions and reports concerning the ſafety, the economy, and the other effects of theſe engines have paſſed under my notice; but the intereſted ſituation of ſome of the narrators on both ſides, and the ſhort time of trial, have induced me to wait for more facts before I ſhould give any account of the engine in this Journal. I hope to do this a few months hence.—W. N.

ciſe

cise value or effect of any mill or engine in and about London. Indeed I do not know why it was adopted for the purposes intended, it being so indefinite.

This unity probably arose from steam engines being substituted for horses.

I can easily conceive it probable that somebody who has employed horses for some time in mill-work, may have applied to an engineer, and said "I have a mind to have my work done by a steam engine instead of horses, for I am to a point that I shall save money by it, and please to give me an estimate of the cost of an engine that will do the work of my horses;" and then the engineer sets about getting information as to what may be deemed the effect a horse can produce, and calls it the *horse power*; and perhaps having Desagulier's Experimental Philosophy at hand, applies to him, and there he finds that a horse will raise a hogshead of water 50 feet high in a minute; then what is the weight of a hogshead of water, and he finds from some particulars related by him, 2nd vol. page 505, that a hogshead of water is equal to 550 lb. but of what measure is uncertain, for the ale hogshead, 51 gallons, is 540 lb. and the wine hogshead, 63 gallons, is 504 lb. reckoning the cubic foot at 1000 ounces avoirdupois.

Desaguliers considers it as 550 lb. raised 50 feet per minute.

Another estimate; nearly double that of Desaguliers.

Some engineers who have very unceremoniously taken the lead in this affair, have adopted I do not know what for a *datum*, but the result is this: An engine by calculating 10 lb. on the square inch, making the whole pressure = 1000, moving through 200 feet per minute, is called a *four-horse engine.*— Let us see then what will be the effect of one horse according to this fact.

$1000^{lb.} \times 200^{ft.} = 200000 =$ the whole effect, then $\frac{200000}{4}$ = the effect of one horse. Now compare this with the estimate of Dr. Desaguliers, which is a hogshead of water at 550 lb. 50 feet high in a minute; 550 lb. $\times$ 50 ft. $= 27500$ and; $50000 - 27500 = 22500 =$ the difference between one of Mr. Watt's horses and one of the doctor's, on the former of which I make no comment.

Smeaton's estimate one-fifth less than Desaguliers.

Mr. Smeaton, whom I hold as having superior claim to precedency on subjects of this nature, has utterly disapproved of Desagulier's experiment by the most powerful conviction of its fallibility, formed by conclusions drawn from sterling experience in the accomplishment of works on a large scale; and he states the greatest effect to be 40 feet high in a minute; but

as

as this is ſtill in the commonly received opinion as to the weight of the hogſhead, I would rather turn to thoſe who have made their experiments on a weight of ſolid matter, expreſſed in terms which cannot be miſtaken.

I remember to have had ſome converſation on this ſubject many years ago, with the late Samuel More, at that time ſecretary to the Society for the Encouragement of Arts, &c. when he ſhewed an inſtrument conſtructed on purpoſe to determine the reſiſtance againſt horſes at plough. I do not recollect that I made any minutes on the reſult of our converſation, ſo can only ſay that his relation of the fact ſurpriſed me, until I came to compare it with the effect of horſes actually applied to overcome a load drawn up a ſhaft in a mine; but I had not the ſame means of determining the re-action that he had; however, the reſult of his experiment may be ſeen in the Tranſactions of that Society; ſome obſervations on it may be ſeen in the 3d vol. quarto, of the Philoſophical Journal, page 136, only there ſeems to be a miſtake in the deduction in the note: it ſhould be 264 : 10 : : 1375 : 52 +

Mention of experiments on the reaction againſt horſes, by the late Sam. More, Eſq. with an inſtrument. *It was a ſpring with graduation.*

I much wiſh to have an experiment like Mr. More's made by a ſledge drawn forwards and backwards on a level road, with Mr. More's inſtrument placed between it and the horſes; ſuch an experiment would be very practicable, and the ſmall deviation from the true level of a road would be compenſated by alternately going firſt one way and then the other.

Propoſed experiment.

It is true that we are become pretty well acquainted with what may be done by horſes in grinding malt, pumping liquor and worts in breweries; but there are ſo many fortuitous circumſtances to be regarded, even here, that nothing decided can reſult from the cloſeſt inveſtigation. For inſtance, ſome brewers chuſe to have their malt ground much lower than others; the pump-work is executed in ſome breweries under very different advantages, and from local circumſtances may be retarded by the inertia neceſſary to communicate motion from the wheel to the work, adding the different condition of valves, buckets, &c. All theſe conſiderations demand ſome *invariable* reſiſtance to be overcome by the exertion of the horſe, and I know of nothing ſo appropriate as that I have juſt mentioned.

Uncertainty of the meaſure of a horſe power from grinding or pumping, &c.

The power of a horſe (by which I mean the mechanic power) is not eaſily aſcertained. It has ſcarce any analogy with

Difficulty of this ſubject, not only from the work, but the

class of horses employed.

with a weight descending through a given space or a quantity of water falling a given height, and therefore is better expressed by the terms effect, resistance, re-action, &c. and even then, to be any thing like precise, we ought to discriminate whether brewers' horses, or higlers' horses, waggon horses or coach horses, heavy horses or light, and if you will go into the country among the coal-mines, you will have another class of these animals, which I know not what to call unless it be *poor horses*, full worked and half starved; in short, I mean that neither one or the other ought to be taken into the account as the *measure of a mechanic power*.

Mr. More's estimate of 80lb. 3 miles an hour. This is nearly three fourths of Desagulier's rate.

Nevertheless it seems desirable to have some popular expression for the application of whatever may be substituted in the place of horses, whether steam, water or wind; nor can there be any objection to saying, "equal to the work of so many horses," provided we can attain to a clear, unequivocal and somewhat exact value, attributable to that power, and if I may give my own opinion, I think Mr. More has stated the utmost effect to be 80lb. three miles per hour, in such horses as are proper for giving motion to mill-work, and at such spells as will not exhaust the breath or strength of the animal.

Remarks.

I am surprised to find this mode of calculation has obtained so far as to determine the power engines employed purely as pumping engines, as lately at the Tunnel, the New Docks, &c. but I am glad it reaches no further than the bills of mortality, and I hear that the Dutch method of hoisting goods to warehouses has lately been adopted at some of our new docks. *O tempora, O mores!* While other countries are availing themselves of the application of the steam engine in place of animal labour, we are taking up the expedients of those who have scarce heard there is any such thing as a steam engine, or who cannot appreciate its value on that degree of evidence we have in our own country.

Whether the horse power or unity be true or not, it ought surely to be free from ambiguity.

It may be objected to by some to alter the present data, however erroneous, as we shall be obliged to require 20 horse engines instead of 10 (for it appears the estimate is nearly, if not quite *cent. per cent.* more than it should be); but even that can make no difference in any respect than as making a rent in an egregious error; the cost of an engine cannot be altered by it, nor the consumption of fuel, but a material convenience would be the result of such a regulation, considering the ad-

S vantage

vantage of a coincidence in this point throughout the kingdom, and as partaking of the nature of a unity of weights and measures, it ought to be paramount to every subordinate consideration.*

Your much obliged obedient servant,

J. C. HORNBLOWER.

VII.

Letter from Mr. A. F. Thoelden, communicating three manuscript Tables from Mr. Bode, of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come.

To Mr. NICHOLSON.

SIR,

THE three planets, or asteroids (according to Dr. Herschel), lately discovered, being so very small, are not easily found, unless the observer is acquainted with the place where he is to look for them. This uncertainty induced some astronomical gentlemen to desire me to inquire, if there were not any ephemeris of their motions published abroad. I complied with their request, and Mr. Bode has very obligingly communicated to me the following written account of their respective situations, calculated for the Observatory at Berlin, (that of Juno, according to a table of his own calculation). If you think this communication may be interesting to the astronomical readers of your Philosophical Journal, I beg you will make use of it.

Tables obtained from Mr. Bode of the places of the new planets.

I am, Sir,

Your most obedient humble servant,

10, *St. Alban's Street*, A. F. THOELDEN.

May 13, 1805.

P. S. A new edition of Mr. Bode's small Celestial Atlas has just been published; with a Catalogue of 5500 Stars, after Piazzi's observations. This last work can be had separate.

Any gentleman who may be desirous of one or both these works, will be supplied in a reasonable time after sending an order to me.

* For a very clear and rational report of a steam-engine in horse-powers, see our Journal, IX. p. 215.—W. N.

TABLE I.

GEOCENTRIC MOTION OF CERES.

1805.	A. R.		Decl.		1806.	A. R.		Decl.	
Oct. 2.	104°	34′	22°	51′N.	Jan. 9.	103°	26′	29°	46′N.
11.	106	53	23	3	18.	101	11	30	25
20.	108	49	23	18	27.	99	18	30	53
29.	110	21	23	39	Feb. 5.	97	56	31	11
Nov. 7.	111	24	24	5	14.	97	13	31	22
16.	111	54	24	40	23.	97	10	31	26
25.	111	49	25	22	Mar. 4.	97	46	31	25
Dec. 4.	111	7	26	12	13.	98	58	31	20
13.	109	48	27	7	22.	100	43	31	11
22.	107	59	28	3	31.	102	57	30	58
31.	105	46	28	58	Apr. 9.	105	32	30	40
					18.	108	28	30	18
					27.	111	40	29	51
					May 6.	115	6	29	19
					15.	118	42	28	40
					24.	122	27	27	56

TABLE

TABLE II.

GEOCENTRIC MOTION OF PALLAS.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Aug. 3.	59° 29′	3° 4′ S.	Jan. 3.	68° 34′	31° 16′ S.
12.	62 47	4 22	12.	67 51	29 27
21.	65 54	5 56	21.	67 48	27 12
30.	68 52	7 47	30.	68 24	24 39
Sep. 8.	71 34	9 53	Feb. 8.	69 37	21 54
17.	73 58	12 15	17.	71 26	19 3
26.	75 59	14 50	26.	73 45	16 10
Oct. 5.	77 35	17 37	Mar. 7.	76 33	13 20
14.	78 40	20 30	16.	79 44	10 35
23.	79 12	23 23	25.	83 16	7 59
Nov. 1.	79 8	26 8	Apr. 3.	87 6	5 34
10.	78 26	28 38	12.	91 11	3 21
19.	77 11	30 43	21.	95 28	1 22
28.	75 29	32 13	30.	99 57	0 19 N.
Dec. 7.	73 33	33 3			
16.	71 35	33 9			
25.	69 51	32 32			

TABLE

TABLE III.

GEOCENTRIC MOTION OF JUNO.

1805.	Longitude.			Latitude.		1806.	Longitude.			Latitude.	
Oct. 1.	4s	28°	22′	6°	21′ S.	Jan. 1.	5s	26°	39′	4°	18′ S.
11.	5	2	28	6	12	11.	5	27	25	3	56
21.	5	6	18	6	1	21.	5	27	29	3	31
Nov. 1.	5	10	23	5	47	Feb. 1.	5	26	45	3	0
11.	5	13	54	5	33	11.	5	25	19	2	22
21.	5	16	44	5	21	21.	5	23	20	1	44
Dec. 1.	5	19	23	5	8	Mar. 1.	5	21	33	1	15
11.	5	21	34	4	54	11.	5	19	0	0	33
21.	5	23	27	4	38	21.	5	16	41	0	10N.
						Apr. 1.	5	14	27	0	50
						11.	5	12	56	1	26
						21.	5	12	5	1	57
						May 1.	5	11	46	2	19

Letter

VIII.

Letter from Professor PINI, *Inspector of Mines to the Italian Republic, to* J. C. DELAMETHERIE, *on Corindon found in Italy.**

THE interest you take in publishing discoveries in natural history in your excellent Journal, induces me to communicate to you a mineralogical rarity lately found on a mountain of the Italian republic: it is a very fine corindon, or adamantine spar, of a deep ruby colour. I saw it for the first time among the minerals which the learned Brochi, professor of natural history at Brescia, had made a short time before in the department of Serio. At the first view he considered it as a feldspar, of which it has all the appearance; and, in fact, the corindon being a substance which hitherto has only been furnished by countries far distant from us, it would have been imprudent to have judged otherwise at first: but the colour of stone, exactly resembling that of a red corindon which I brought from Paris, given to me as coming from Madras, led me to suppose that it did not differ from it. Beautiful red adamantine spar found in Italy.

But as Professor Brochi purposed meeting me in a short time at Milan, to which place I was going, we postponed the verification of this suspicion. When he saw the red corindon from Madras, in my possession, he no longer doubted the identity of its species with that of our sample. I afterwards discovered the same identity in the trials to which I submitted it: the following are the results:

1st. The corindon of Italy scratches the hardest rock-crystal. 2d. It does not melt before the blow-pipe, either alone or with the addition of borax. 3. Its texture is in laminæ, which follow different directions. 4th. Its fissure is triple, and when it is cut in the three directions, it offers a rhomboid, the acute angle of which is 64¾°. 5th. Its cross fracture shews the splendor of the diamond, and reflects the light, the flashes of which are almost the colour of silver. 6th. Its specific gravity is 3.87, which is the mean of that of the true corindon. Examination. 1. Hardness. 2. Infusibility. 3. Laminar texture. 4. Fissure and angle. 5. Reflection of light. 6. Sp. gravity.

Hitherto it has been met with in a mountain of micaceous schistus, in pieces of several inches in length, which are amorphous Is found on a mountain of micaceous schistus.

* From Journal de Physique, Vendemiaire, An XIII.

phous and opake, but femi-tranfparent on the thin edges. Profeffor Brochi and I purpofe making new refearches there, which may lead to fome more interefting difcovery.

Countries where corindon is found.

In the mean time it will no longer be doubted, that the corindon is a product of Europe. M. de Bournon, with whofe memoir on corindon, inferted in the Journal des Mines, Vol. XIV. you are well acquainted, has noticed the different countries which furnifh this fubftance; they are the ifland of Ceylon, the peninfula of India, and in particular Madras, the Carnatic, and China. He concludes his interefting details by enquiring whether this fubftance exifts in other countries, except thofe acknowledged to be the chief, if not the exclufive fituations of this fpecies. This queftion arifes from feveral ftones found in Europe having been given as corindons. In fact, thofe collected in Germany were found to be fometimes feldfpars, and fometimes the *fchorlartiger-beryll* of Werner, your leucolite. That mentioned in the Mufeum Brittanicum as coming from Tyrie, on the eaftern coaft of Scotland, was far from having the hardnefs belonging to this fpecies; that from Chefnut-hill near Philadelphia, announced by Mr. Smith, was difcovered by Mr. Richard Phillips to be a fragment of badly cryftallized quartz. It only remained to decide on the feldfpar found by Bournon in France, in the province of Forez, the defcription of which he fent to you in a letter inferted in the Journal de Phyfique for June 1789, and which he ftill confiders as a true corindon.

The fpecimens of European ftones called corindon do not belong to that fpecies.

This fubftance appears to be the fame as that you have called andaloufite, and which fome dealers in natural hiftory have circulated in commerce by the name of adamantine fpar, from the kingdom of Caftile: it has been placed by Abbé Hauy in the appendix, which contains thofe fubftances, the nature of which did not appear to be fufficiently known to permit him to affign them a place in his method. He calls it apyrous feldfpar. Thus we may be fatisfied that hitherto there is no certainty of corindon having been found in Europe. I flatter myfelf that now there will be no doubt on the corindon of Italy which I have the honour to announce; it is not even deficient in the fpecific gravity belonging to this fubftance; a defect which induced Profeffor Hauy not to acknowledge the feldfpar of Forez to be a corindon.

If

If rubies and sapphires be classed with corindon, as many mineralogists seem disposed to do, Europe will probably have mines of these precious stones as it already has mines of emeralds, such as those of Limoges, discovered by the learned Lelievre, counsellor of the mines. But these stones will be rubies, sapphires, and emeralds of mineralogists, but not of the jewellers, until they shall be found very transparent, which I hope will be the fruit of new researches, followed with perseverance.

Probability that Europe may afford the precious gems.

HERMENEGILDE PINI.

IX.

On disclosing the Process of Manufactories. In a Letter from Mr. JOHN CLENNELL.

To Mr. NICHOLSON.

MY DEAR SIR, *Newcastle, Feb.* 17, 1805.

PERMIT me to intreat the attention of some of your numerous correspondents towards a question which must certainly be interesting to every manufacturer, but of which no regular discussion has yet been offered—Is it proper or improper to lay before the public in respectable periodical works, a full and impartial statement of the various processes of our manufactories? I shall state such reasons as have offered themselves to me why they should be displayed, but I am principally anxious to receive further information on a subject that appears to me peculiarly interesting.

Interesting question respecting the disclosure of manufacturing processes.

The first argument I shall adduce is that of Mr. Boyle, as quoted by Dr. Johnson in the 201st number of the Rambler. "The excellency of manufactures, and the facility of labour, would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another with some peculiar advantages, which, though *singly* of little importance, would, by *conjunction and concurrence*, open new inlets to knowledge, and give new powers to diligence."

Argument of *Boyle*,

The

confirmed by experience.

The second is the very considerable improvements that have taken place in those few manufactories which have yet been under the influence of chemical enquiry; thus realizing, but on a very extensive scale, the suggestions of Mr. Boyle: so far therefore as we are to be guided on the one hand by experience, and on the other by the influence of scientific enquiry on liberal display, will the argument be in our favour.

Accidental discoveries improved by disclosure.

In the third place I would observe, that as many very valuable discoveries are owing to accident, those with whom they happen are frequently perhaps incapable of improving them to the extent they would admit of in the hands of men of science, and thus, by a spirit of monopoly, preclude even themselves from the advantageous cultivation of such discoveries, merely lest others might enjoy it also. If, again, we consider the rapid progress that has been made of late years in every department of useful and practical knowledge, we must attribute it entirely to those liberal communications that have been made by men whose attention has been immediately directed to the promotion and improvement of every thing valuable to the public. Again, the profits of every business depend on the regularity and knowledge with which it is conducted; but how is the last to be enjoyed without resources to apply to, and how much more easily would it be obtained if science could regulate and simplify the combinations of the manufacturer? If to accomplish by every thing employed (and even in many cases the refuse) in each process its utmost possible use, is a favourite principle with manufacturers; to take the most accurate and best adapted means to effect it, ought certainly to be as powerful with them. Is it not also obvious, that, to discard all mystery and quackery, and fairly to disclose each process, is to invite the attention of men of science and research to extend any advantages gained by chance or otherwise, and to discover greater utility in the various substances employed. The origin, progress, present state, and hints for the improvement of the "arts of life," would certainly be worthy the contemplation of our first chemists, and are subjects that have appeared of such importance to a neighbouring nation, that many of their most eminent men have been employed in them; and some volumes of the *Encyclopedie Methodique* are dedicated to such information, with plates too, in several instances displaying even the most minute work-tools employed in each.

Science would be thus introduced into workshops, &c.

The

The history and detail of manufactories conducted in each place ought, I presume, to form a principal object with the writers of local histories; yet very few of those gentlemen are enabled to obtain such accounts as they can depend on, from the selfish and monopolizing jealousy of manufacturers in general. To these various advantages an objection may be offered, "That display is placing objects of taxation in the hands of ministers: be it so; display will make it easier to collect the tax, will make it more certain, and it may be, *less oppressive:* if to these be added the above advantages, it may fairly be presumed, that discovery and consequent improvement is the most advantageous track to be pursued; but, my dear Sir, I beg your pardon, on this subject I did not mean to offer my own opinion so much as to solicit information from that of others. Some objections answered.

I am truly your's,

JOHN CLENNELL.

How far literary pursuits are compatible with the duties of the commercial man, or the manufacturer, seems a question so completely decided in the affirmative, in the first volume of the Manchester Memoirs, by Mr. Henry; in the second volume of the same work, by Dr. Barnes; and in the hundredth number of the Lounger,—that the above paper assumes the principle as being fully established. J. C. Literary pursuits are compatible with the duties of merchants and manufacturers.

X.

Question respecting the Purification of Copper. By J. P. *With a concise Reply.* W. N.

As copper in its purest state (especially out of London) for manufacturing different articles, cannot be obtained without a very tedious process; as it forms the principal ingredient in mirrors for reflecting telescopes, and likewise is much used as an alloy for gold; if it is impure, it never fails to render the gold so alloyed brittle, and not to be restored to its ductility until the impure alloy be wasted by subsequent meltings, to the loss and disappointment of the workman. Required, therefore, Question respecting the purification of copper.

therefore, a method of purifying the copper, particularly for the latter purpose?

Your's respectfully,

Morley Street, J. P. *Jun.*
Newcastle-upon-Tyne.

Reply.

As the processes for refining copper in the large way are grounded upon its property of resisting oxidation more than the other metals which are usually combined with it, it may be adviseable to adopt the process of Pelletier, with a due attention to the manipulation and the proportion of manganese to be made use of. The very interesting letter of Mr. Thomson in the present Number, will indicate the principles of operation. Mr. Hatchett's excellent papers in the Philosophical Transactions, of which a correct abridgement is given in our V. and VI. Volumes, shew the mischievous consequences of impurity in the copper for alloying the precious metals; and it is but too well known, that it is difficult to be procured, or even to be made pure, upon a scale of extensive magnitude.

W. N.

XI.

Description of a Method of connecting Iron Bars, and coating them with Lead, so as to form solid Pillars for Light-houses on Rocks covered at High-water, and to defend them from Corrosion. By Capt. JOSEPH BRODIE, *of the Royal Navy.**

Description of the means of coating iron bars: by reference to the drawing.

FIG. 1, *Plate V.* A shews four rods of cast iron, composed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, so as to produce the effect of one bar of the thickness of the whole. B. A tube of cast iron, formed from a number of separate pieces, each about ten inches long, and which, when placed round the iron rods above-mentioned, and then screwed together, form a mould, into which the melted lead is to be poured, to coat the iron rods.

* Communicated to the Society of Arts (Memoirs, MDCCCIV. 258.) who voted him the gold medal.

rods. C. A portion of the rods covered with the melted lead, so as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

Fig. 2. D shews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of semi-cylinders rivetted together and fitting each other. E, the side flanges screwed close together. F, the end flanges also screwed together, as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will answer the purpose to coat any length of the iron rods.

XII.

Reply to Mr. ACCUM's *last Letter on the Production of Nitrous Acid. By* W. F. S.*

To Mr. NICHOLSON.

SIR,

I THINK Mr. Accum by no means throws off the charge of censure with which he is accused, upon so material a part of the modern theory. He ought to have been more explicit, and in so nice and so disputed an experiment, to have given us a minute detail. Remarks on Mr. Accum's letter.

I have performed the experiment which he speaks of, but I could detect no nitrous acid after the process. The air gradually diminishes by the electric spark, but this diminution is owing to the oxigen gas producing a calcination of the metals employed, for the purest oxigen gas answers better than when mixed with nitrogen. Therefore I hope, if Mr. Accum possesses a more accurate experiment, he will give it through your Journal: certainly the present state of modern chemistry requires a very minute investigation. I hope, Mr. Nicholson, you will not refuse inserting this in your Journal. Unsuccessful attempts to repeat the experiment of forming nitrous acid.

London, May 2.

* See our Vol. X. p. 109 and 214.

Experiments

XIII.

Experiments on the Electricity of Metallic Filings sifted through Metal; with Remarks in answer to a Letter of Mr. Cuthbertson. By Mr. Wm. Wilson. (*From the Author.*)

To Mr. NICHOLSON.

SIR.

An error pointed out by Mr. Cuthbertson admitted.

IN the last number of your Philosophical Journal, I find a letter from Mr. Cuthbertson, containing some remarks on my letter on the electricity of metals, in which he notices an error in the table of results of experiments, that had escaped my notice till I read his letter. In that table I have by mistake put P against copper filings sifted through zinc instead of N.

Copper filings when sifted through zinc are electrified strongly with negative electricity.

Mr. C.'s objection to the production of electricity by mere separation of metals, considered. The metallic filings *touch* the sieve, are *separated*, and *touch* the receiving metal. As the el. varies with the sieve it does not depend on the *latter contact*, —and in the *first contact* there is no insulation, —consequently it depends on the *separation*.

Mr. C. in his other remarks does not seem to admit that the separating the metals from contact is the cause of the electrical fluid being excited and not touching, because both touching and separating are employed. In the way the experiments were made touching takes place two ways, viz. the filings come in contact with the metal plate they are sifted into, and they are in contact with the sieve before they are sifted through it; but I cannot conceive how either of these contacts could cause the excitation of the electricity in the experiments. If the contact of the filings with the plate they were sifted into was the cause, we should have had the same effect with the same filings, whatever metal the sieve was made of they were sifted through; because as they were always sifted into the same plate, the same metal filings always came in contact with the same metal plate, and consequently we should have had the same effects in all cases with the same filings; whereas out of the ten sorts of filings that were used there was only one (steel) that produced the same effect with the different sieves; and if the contact of the filings with the sieve excited any electric fluid, it would be dissipated as fast as excited, because neither the sieve nor the filings were insulated; consequently it could have no effect on the results of the experiments.—Therefore since neither of these contacts excited the fluid, it must have been excited by the separation.

I think

I think the following experiments will put this beyond a doubt: 1. I faftened a piece of card into a ftick of glafs, and then rubbed it over with ftrong gum water and covered it with filings of zinc; fo that when it was dry, it had a furface of filings of zinc. From a heap of the fame filings I took up as much as I could on this little fhovel without touching it with any thing elfe, and let them fall very flowly upon a piece of bright fheet copper faftened in an inclined pofition upon the cap of an electrometer, and formed into a receptacle at its lower part to contain the filings. In this operation almoft every particle of filings neceffarily came in contact with the face of the copper. After letting fall upon the copper about an ounce and a half of filings there was not the leaft fenfible effect on the electrometer. 2. I then took a piece of the fame fheet copper, which was pierced full of fmall holes, and fifted through it the fame filings I had ufed in the above experiment, upon the fame copper on the electrometer, and the gold leaves diverged with pofitive electricity and difcharged themfelves againft the flips of tin foil on the infide of the glafs ten times before the whole of the ounce and half of filings were fifted into it.

An infulated fhovel of card, faced with zinc filings was ufed to pour zinc filings on a bright plate of copper.

The copper plate was not electrified.

A copper fieve was then ufed inftead of the fhovel.

The copper plate was ftrongly pofitive.

Now, fince the feparating the two metals from contact in the fecond of the above experiments is the only difference between them, and as the electric fluid was excited only in the fecond, I think we may fafely conclude that that feparation was the caufe of the excitation, and not touching.

Separation was alone the caufe of this electricity.

I am very much inclined to believe that the excitation that takes place in friction is caufed by the fame circumftance, and that the friction does nothing more towards the excitation than bring the different parts of the fubftance that are rubbed together into contact, and feparate them from it.

Electric excitation is probably a fact of this kind.

I am your obedient humble fervant,

WILLIAM WILSON.

Reply

XIV.

Reply to Mr. Bofwell. *By* An Old Correspondent.

To Mr. NICHOLSON.

SIR,

Explanatory remarks on Mr. Bofwell's letter.

WHEN I began to read your correfpondent, Mr. Bofwell's anfwer to my obfervations on his geometrical propofitions, and found myfelf accufed of having " thrown fome very undeferved reflections" on his communication, I could not help feeling a degree of apprehenfion left I fhould inadvertently have made fome miftake or other, for which I muft have been obliged to apologize to him and to the public; but on perufing the letter through, I was not a little furprifed to have found but *one* reflection pointed out, and that applying not to the *matter* but *manner* of my obfervations; it feems I have accufed him of being *too confident* in one of his affertions; a literary crime, of which he exculpates himfelf by proving from a quotation, certainly very much to the purpofe if we make no diftinction between *doubt* and *diffidence*, that he is, on the contrary, a very *diffident writer*, which quotation, to be fure, contrafted with an expreffion that fell from my pen, might be conclufive, if the two paffages were applicable to the *fame thing*; but unfortunately it turns out, on clofer examination, that the accufation applies *exclufively* to Mr. B's *fecond* propofition, and his exculpatory quotation *exclufively* to his *firft*.

Had I committed myfelf fo far as to fay that he announced his firft propofition with confidence, I muft have ftood clearly convicted of having done him injuftice; but as it was his fecond propofition, or, in other words, his *other fact* in geometry, to which my objectionable obfervations *folely* applied, he ought to have quoted what he has faid about *it*, and about *it only*, as evidence againft me; he has, however, directed his arrow at a wrong mark, on which account I claim the reader's indulgence to repeat the paffage alluded to, which is the *only* paffage in Mr. B's paper that relates to his fecond propofition: " The difcovery of a fact in geometry often leads to *another;* one of this kind *I have here to add, which is,* that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppofite diameter (I F) to the circumference,

ence, *will be equal* to a fourth of the circumference." In this annunciation, which is an appendage to the main subject or first proposition, and evidently not included in the prefatory apology, I see nothing like a suspicion of inaccuracy expressed, or even hinted at, and consequently can trace no mark of *diffidence*, now that I come to examine it again; on the contrary, I repeat, that a fact is confidently asserted to exist, which has been proved *not to be a fact*, both by myself and by Mr. Gough, to whose testimony probably the reader will pay some deference, particularly as this gentleman has ingeniously shown that *another line* in the circle possesses the identical property erroneously attributed to the line B E, and that previously to Mr. B.'s complaint being made public: to the reader, therefore, the falseness or justice of my observation or "reflection" must be referred; and it will answer the purpose of both Mr. B. and myself, if he will let the affair drop here; for *he* will then stand a good chance of being reputed what is his principal aim, a *diffident writer*, and I shall cease to be, what some of your readers, besides Mr. B. may possibly be disposed to think me, a *caviller*. Explanatory remarks on Mr. Boswell's letter.

I am, Sir, once more,

AN OLD CORRESPONDENT.

May 17, 1805.

P. S. The mathematical portion of your readers need not be informed, that the *forgetfulness* imputed to me, respecting the assumed *approximation* substituted for the *exact ratio* of the diameter to the circumference of a circle, is a charge applying with equal propriety to every mathematician who has deduced calculations depending on the area of a circle: even Mr. Gough, whose mathematical skill is justly the admiration of thousands, whom he has never seen, and is doomed never to see, has somehow been *obliged* to be guilty of the same want of recollection, though his calculations may be considered as the result of *demonstrative truth*.

It remains now for Mr. Boswell to show the scientific world, by an example or two, how he applies his discovery to practical measurements, which it is presumed, will prove a communication of general interest.

XV.

Description of an accurate Method of banking the Balance of a Time-keeper. By Mr. WILLIAM HARDY. *Extracted from his Letter to* CHAS. TAYLOR, *Esq. Sec. to the Society of Arts.**

SIR,

Importance of the stop or banking piece used to prevent extreme vibrations in time-pieces.

THIS letter is accompanied with a drawing, a description, and a model, of a more perfect mode of banking the balance of a time-keeper, than any that has yet appeared; and its application to a time-keeper is a matter of such real importance, that the most accurate, without this most necessary appendage, is liable to such derangement, that from the most trivial cause it is in one moment rendered useless.

The author's invention is confirmed by long trial,

To preserve the good qualities of the time-keeper, on which often the strength, the wealth, the grandeur, and safety of this great empire depend, I deem it necessary that my invention should be laid before the Society of Arts, as the means of its being more generally known; and I hope that I shew proper respect to the Society, when I assure you that I do not offer any crude idea, neither could I think of giving you any trouble, until I had fully verified the utility of my contrivance by several years trial. As I can produce the testimony of some of the most eminent watchmakers in favour of my invention, I look forward with some degree of confidence, in expectation of obtaining the approbation of the Society.

and testimony.

The banking is required in watches which have a vibration through very great arcs.

It was at first imagined, that a banking to a watch with a free escapement was quite unnecessary, as the limits of banking were so great as to admit of almost twice 360, or 720 degrees; but, on trial, the balance was frequently found to exceed this quantity, and that a very slight motion given to the time-keeper (particularly when the axis of the balance became the axis of that motion), was sufficient to alter the strength and figure of the pendulum-spring, and position of the pieces in respect of the balance-wheel, so as to change the rate of the time-keeper; and, what was worse, require a new

* In their Memoirs for 1804. A premium of 30 guineas was awarded for this invention.

new adjuſtment of the balance, to accommodate itſelf to the changes made in the ſpring, and other parts connected with it. Hence it became neceſſary, that ſome means ſhould be uſed to ſtop the balance at certain limits beyond its natural arch of vibration; and various attempts have been made to effect it. One way is, by a moveable piece on the axis of the balance, which banks againſt a pin, yet ſo as to ſuffer the balance to vibrate more than 360 degrees. Another method is to have a piece moveable on a centre in one of the arms of the balance, and applying itſelf as a tangent to the pendulum-ſpring, which paſſes through a hole in the piece. It has alſo a knee, which almoſt touches the plate, and juſt paſſes free of a pin placed in it. But when the balance vibrates ſo as to approach its utmoſt limits, the action of the ſpring, while in a ſtate of unwinding, throws the piece outward, ſo as to fall in the way of the pin, and ſtop the balance from proceeding farther. Another mode is by a ſtraight ſpring, ſcrewed upon the plate, having a hook at the end of it, into which a pin placed in the balance ſtrikes, when, as before, the pendulum-ſpring, in unwinding, touches the ſtraight ſpring, and moves it a little outwards. There is alſo a way of banking by means of a bolt, which is thrown back by the pendulum-ſpring, and made to fall in the way of a pin placed in the rim of the balance. Theſe are the principal modes of banking now in uſe, and they do not differ materially from one another in principle. But the weight and friction of ſo many pieces, on ſo delicate an organ as that of a pendulum-ſpring, are perhaps nearly as hurtful to the time-keeper as the injury it may ſuſtain when it is left without any banking whatever.

Former methods of banking.

They are objected to from weight and friction.

In *Figures* 1 and 2, *Plate VI.* the ſame letters are placed, to ſignify the ſame things. A A is the balance to which the pendulum-ſpring is faſtened in the uſual way. In one of the croſſes of the balance is placed a pin P, which ſtands a little way above its ſurface; and when the balance is cauſed to vibrate a complete circle, the pin in its motion will deſcribe the dotted circle P O Q, and juſt paſs clear of the inſide of a projection formed on a cock B, which is faſtened on the plate by means of a ſcrew. At about one-fourth of a turn of the pendulum-ſpring, reckoned from its ſtud E, is placed a very delicate tapering piece of ſteel S, having a ſmall hole in

Deſcription, with reference to the engraving.

Defcription, with reference to the engraving.

it, through which the pendulum-fpring paffes; and it is faftened to it by means of a pin, and ftands perpendicular to the curve of the fpring. Let the balance be at reft, as reprefented in *Fig.* 1, the banking-pin at P, and the banking-piece at *s*. Suppofe the balance is made to vibrate from P towards O, when P arrives at the banking-piece *s*, it will pafs it without touching, becaufe its extremity *s* lies wholly within the circle traced out by the banking-pin. But when the banking-pin P has arrived at Q, the banking-piece *s* will have advanced to *t*, by the pendulum-fpring winding itfelf up into the figure reprefented by the dotted curve; and when the banking-pin P (now at Q) returns back to P, and paffes on from P towards Q, to approach B, and fo complete the other half-arch of its vibration, before P can arrive at the banking-cock B, the pendulum-fpring will have unwound itfelf into the figure defcribed by the dotted curve, and the banking-piece *s* will have advanced into the pofition at *r* juft touching the banking-cock. Its extremity *r*, however, being thrown beyond the dotted circle, muft neceffarily fall in the way of the banking-pin, which arrives there almoft at the fame moment, and is oppofed by it, without the flighteft fhock to the pendulum-fpring. The model* renders any farther explanation unneceffary.

WILLIAM HARDY.

No. 61, *Chapel-Street, near White-Conduit-Houfe,*
Jan. 18, 1804.

XVI.

Defcription of a new Apparatus for making the gafiform Oxide of Carbon. Communicated by Mr. DEYEUX.†

The progrefs of chemiftry owing chiefly to improved apparatus.

IT is generally admitted, that chemiftry is indebted to the invention of various kinds of apparatus, and the perfection to which they have been brought, for much of the progrefs it has made within thefe laft thirty years.

Inftance in that of Woulfe.

For inftance, before Woulfe made known his apparatus for obtaining the aeriform fluids evolved from various fubftances,

* Which is preferved in the collection of the Society.
† Annales de Chimie, Vol. LIII. p. 76.

either

either when exposed to the action of fire, or when brought into contact with matters capable of combining with them, the operator was obliged to employ bulky vessels, difficult to manage, and so inconvenient, that he did not wish to apply them; and when he did, it was impossible for him to collect the aeriform fluids with any accuracy; for when they were rarefied to a considerable degree, they escaped through apertures left purposely to preserve the vessels from bursting.

Inconvenience of the old instruments.

These inconveniences are now removed by Woulfe's apparatus, so that the operations in which gases are evolved may easily be performed in vessels of small bulk; the gases may be subjected to calculation; their quantity as well as quality ascertained with the utmost precision; and operations, which were formerly considered as very hazardous to the operator, may now be continued for hours together without the least fear of injury.

Contrasted advantages of the modern arrangement of vessels.

With these advantages many others are connected; and it is known to every one, that they are owing to the degree of perfection to which chemists have brought Woulfe's apparatus, and particularly to their happy application of it on various occasions.

Yet, notwithstanding these discoveries have been carried a great way, it is more than probable that many remain to be made: too much praise therefore cannot be bestowed on those who turn their attention to this important object, since the apparatuses they invent are so many new means afforded chemists of collecting an infinite number of products which frequently escape them, and the knowledge of which may have great influence on the improvement of chemical science.

Improvements yet remain to be made.

From these motives I have thought it may be of use to make known an apparatus just invented by Mr. Baruel, operator to the chemical lectures of the Medical School at Paris.

Invention of Mr. Baruel;

This young chemist, who had often noticed the difficulties and even dangers incurred when it is necessary to decompose gases, or to combine them with different substances, attempted to make some alteration in the processes commonly employed in the laboratory for operations of this sort; and after several trials he invented an apparatus, which succeeded beyond his hopes.

for decomposing gases, or combining them with other substances.

I have seen this apparatus employed with the greatest success for the fabrication of the gasiform oxide of carbon. This gas,

Gasiform oxide of carbon formerly procured with difficulty,

is now easily made.

gas, which formerly was to be procured only with difficulty and in small quantity, may now be obtained easily, readily, and without much expense, which affords the advantage of subjecting it to many more experiments than has hitherto been done.

Other uses of the apparatus.

The apparatus in question may be of use likewise for the preparation of sulphurated hydrogen gas, carbonated hydrogen gas, and phosphorated hydrogen gas: it may likewise be employed for saturating a gas with any substance whatever, particularly when a high temperature is requisite for this saturation.

The better to make known the apparatus of Mr. Baruel, I will give the description of it communicated to me by the author, and add a sketch of it, which will give a more complete idea of all its parts. *Plate VIII*.

Description of the apparatus, and method of procuring gasiform oxide of carbon.

Suppose it is required to make gasiform oxide of carbon: some charcoal is to be taken very dry and carefully chosen, broken into small pieces, and introduced into the three gun-barrels, B, C, D. The charcoal is to be pressed lightly together with an iron rod, so as to occupy only the part of each barrel that is to be heated, but it must not be rammed hard. The three gun-barrels are then to be placed horizontally side by side in a reverberatory furnace, A, leaving about two inches distance between them; secured in their places with moistened brick earth, and covered with the dome of the furnace.

This done, fix in the mouth of the barrel B the glass tube E, which is curved so as to admit its other extremity to be inserted into the neck of the bottle F; the neck of this bottle being large enough to hold likewise the pipe of the curved funnel G. Into the opposite end of the barrel B one of the ends of the curved tube H is to be introduced, the other end of the tube being inserted into the opening of the barrel D, so as to form a communication between B and D. A similar tube I is fitted to the other extremity of D and the adjacent end of C, forming a communication between these two barrels. Lastly, From the opposite extremity of the barrel C issues the tube K, which is bent at right angles, so that its second curvature passes under the receiver M, placed on the shelf of the pneumatic tub with water L.

Every

Every thing being thus arranged, carbonate of lime diluted with a ſmall quantity of water is to be poured into the bottle F, and after all the joints of the tubes have been luted with great care, a fire is to be kindled in the furnace. As ſoon as this fire is ſufficiently ſtrong to make the gun-barrels red-hot, ſulphuric acid is to be poured into the funnel G, and when it comes into contact with the carbonate of lime in the bottle F, it will expel a large quantity of carbonic acid. This acid preſently paſſes through the tube E into the barrel B, is conveyed from B to D through the tube H, from D to C through the tube I, and thence iſſuing by the tube K, comes out beneath the receiver M, placed on the ſhelf of the pneumatic tub.

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

The object propoſed by Mr. Baruel in this arrangement of his apparatus, was to oblige the carbonic acid gas evolved from the carbonate of lime, to traverſe the charcoal contained in the three gun-barrels, and thus ſaturate itſelf with all the carbon it could take up.

In fact it is eaſy to conceive, that this method muſt be more certain and expeditious than that formerly employed, when the operator was ſatisfied with paſſing the gas through a ſingle barrel. It is true it was collected, and ſubjected to a ſecond operation, or even to a third; but this mode was tedious, and much of the gas was always loſt. In this new method on the contrary nothing is loſt, and a product is ſeparated at once, which poſſeſſes all the properties that characteriſe the gaſiform oxide of carbon, and which may be uſed unſparingly, ſince it is always obtainable in large quantity.

This method more certain and expeditious than the old;

for nothing is loſt, and the product is obtained at once.

XVII.

Deſcription of an improved Mill for levigating Painters Colours. By Mr. James Rawlinson, *of Derby.**

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poiſonous and noxious colours, led me firſt to imagine a better might eaſily be contrived for that purpoſe. It muſt be obvious to every perſon, that the method

Great inconveniences and unwholeſome effects of grinding colours on the ſtone.

* From the Memoirs of the Society of Arts for 1804 who awarded him the ſilver medal.

method hitherto adopted of grinding colours on an horizontal marble ſlab, with a ſmall pebble muller, requires the body of the perſon who grinds to bend over that ſlab, and conſequently his head; which cauſes him conſtantly to inhale the noxious and poiſonous volatile parts of the paint, which is not unfrequently ground with oil ſaturated with litharge of lead; and if we may judge from the very unhealthy appearance of theſe men, accuſtomed to much colour-grinding, it ſhould ſeem the bad effects of this employment require a ſpeedy remedy.

Machine by which the work is much better performed, and without inconvenience.

The machine, of which I now ſend the Society a model, has not only the advantage of being an effectual remedy of this extenſive and ſevere evil to recommend it, but it grinds the colour much eaſier, much finer, and much quicker, than any method hitherto adopted. Having occaſion for a conſiderable quantity of colour-grinding in the profeſſion in which I am engaged, and that in the fineſt ſtate poſſible, and having made uſe of this machine for ſeveral years, and being more and more convinced of its utility, I thought it my duty to preſent it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I uſe is ſixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works againſt covers one-third of that roller: it is therefore evident, that with this machine I have ſeventy-two ſquare inches of the concave marble muller in conſtant work on the paint, and that I can bring the paint much oftener under this muller in a given ſpace of time, than I could by the uſual method with the pebble muller, which is ſeldom more than four inches in diameter, and conſequently has ſcarcely ſixteen ſquare inches at work on the paint, when my concave muller has ſeventy-two. I do not mean to ſay that a roller, the ſize of that which I now uſe, is the largeſt which might be employed; for truly I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be ſtill farther increaſed.

It works with five times the ſurface of the muller,

and is applicable to water-colours as well as thoſe in oil.

This machine will be found equally uſeful for the colours ground in water, as for thoſe ground in oils; and I doubt not but the great importance of this ſimple machine will be very ſoon generally experienced in all manufactories where colours are uſed. The labour neceſſary with this machine, in grinding

ing colours exceedingly fine, is very easy. It is useless to enter into any minute description in this place, as a bare inspection of the machine must sufficiently explain itself.

To the colourman it would evidently be an essential saving of labour, and consequently of expense, which will probably have some weight as a recommendation; and the advantages to the colour-grinder have been already stated.

Plate V. Fig. 3. A is a roller or cylinder made of any kind of marble; black marble is esteemed the best, because it is the hardest, and takes the best polish. B is a concave muller covering one-third of the roller, of the same kind of marble, and fixed in a wooden frame *b*, which is hung to the frame E at *i i*. C is a strong piece of iron, about an inch broad, to keep the muller steady, and is fixed to the frame with a joint at *f*. The small binding-screw, with the fly nut, that passes through the centre of the iron-plate at *c*, is for the purpose of laying more pressure on the muller, if required, as well as to keep it steady. D is a taker-off, made of a clock-spring about half an inch broad, and fixed in the manner of a frame-saw in an iron frame *k*, in an inclined position to the roller, and turning on pivots at *d d*. G is a slide-board to draw out occasionally, to clean, &c. if any particles of paint should fall from the roller, and which also forms itself for the plate H, to catch the colour on as it falls from the taker-off. F is a drawer, for the purpose of containing curriers shavings, which are the best things for cleaning paint-mills.—E is the frame.

Description, by reference to the drawing.

Previous to the colour being applied to the mill, I should recommend it to be finely pulverized in a mortar, covered in the manner of the chemists when they levigate poisonous drugs *. This process of dry-grinding is equally necessary for the marble slab now in use; after which it should be mixed with oil or water,

Pulverization, or dry grinding, previous to levigation.

* Or rather in an improved mill, used at Manchester by Mr. Charles Taylor, for grinding indigo in a dry state, of which I have annexed a drawing, and reference, to render the whole business of colour-grinding complete.—*Note of the Author.*

This is the same apparatus as was used under the name of *a philosophical mill*, in the laboratory at Gottorp, about the beginning of the last century. See the memoir of Dr. Joel Langelot, with an engraving, in Lowthorp's Abridgment of the Philosophical Transactions, III. 318.—W. N.

water, and with a fpatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller fpread it equally over its furface. When it is perceived fufficiently fine for the purpofe required, it is very eafily taken off by means of the taker-off defcribed, which muft be held againft the roller, and the roller turned the reverfe way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you defift or change the colour. It is then turned back, being hung on pivots to the frame at *i i*, and cleaned with a pallet-knife or fpatula very conveniently. Afterwards, a handful of curriers fhavings held on the roller, with two or three revolutions cleans it effectually; and there is lefs wafte with this machine than with any marble flab.

Method of working.

Quantity and finenefs of grinding.

As to the quantity ground at once on this mill, it muft be regulated by the ftate of finenefs to which it is required to be ground. If it is wanted to be very fine, a fmaller quantity muft be put on the roller at a time; and as to time requifite for grinding a given quantity of colour, this will alfo depend on the ftate of finenefs to which it is ground. I have obferved that my colour-grinder has ground the quantity of colour which ufed to ferve him per day, with this machine, in three hours, and, as he faid, with eafe. The colour alfo was much more to my fatisfaction than in the former way, and attended with lefs wafte.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent wafte, and prevent the duft and fineft parts of noxious colours from being injurious to the grinder. In fome manufactories, where large quantities of colours, prepared from lead, copper, and arfenic, are ufed, this precaution is particularly neceffary. I do not mean to fay that my machine is intended to fuperfede the paint-mill now in ufe for coarfe common colours. It is intended for no fuch purpofe; but to fuperfede the ufe of the very awkward and unmechanical marble flab now in ufe, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artifts, &c. &c. are now ground.

This mill is not a crude project, but has been ufed feveral years.

Several of the colour-manufacturers have expreffed to me their great want of fuch a machine; and that I had no defire of troubling the public with a machine that would not anfwer, is

is evident, from my having used it several years before I presumed to recommend it to their attention. Being therefore now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to see its ultimate success. And, farther to give the Society the most complete assurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine who has frequently seen it work. If any other questions should occur to the Committee, that may be in my power to explain, I shall gladly do so.

I am, Sir,

Your most obedient servant

JAMES RAWLINSON.

Derby, Feb. 6, 1804.

Charles Taylor, Esq.

Instructions for tying up the bladders of colour.

P. S. When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the usual method. Instead of drawing the neck of the bladder close, in the act of tying it, insert a slender cylindrical stick, and bind the bladder close around it. This, when dry, will form a tube or pipe, through which, when the stick is withdrawn, the colour may be squeezed as wanted, and the neck again closed by replacing the stick. This is not only a neater and much more cleanly mode than the usual one of perforating the bladder; and stopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour; but the bladder, being uninjured, may be used repeatedly for fresh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the stick, into the neck of the bladder, with its closed end outwards, which will keep the colour secure in travelling, and when used, the end of the quill being cut off, it may afterwards be closed by a stick.*

* A certificate from Mr. Thomas Swanwick, of Derby, and also from Mr. John Middleton, of St. Martin's Lane, confirming the above statement, accompanied these papers.

Reference

Reference to the improved Mill for grinding Indigo, or other dry Colours.

Defcription of the mill for dry grinding.

Plate V. Fig. 4. L reprefents a mortar made of marble or hard ftone; one made in the common way will anfwer. M, A muller or grinder, nearly in the form of a pear, in the upper part of which an iron axis is firmly fixed, which axis, at the parts N N, turns in grooves or flits, cut in two pieces of oak projecting horizontally from a wall, and when the axis is at work, are fecured in the grooves by iron pins, O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight which may occafionally be added, if more power is wanted.

Fig. 5. fhews the muller or grinder, with its axis feparate from the other machinery; its bottom fhould be made to fit the mortar. S is a groove cut through the ftone.

On grinding indigo, or fuch fubftance, in a dry ftate, in this mill, the muller being placed in the mortar, and fecured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo in lumps falls into the groove cut through the muller, and is from thence drawn under the action of the muller, and propelled to its outer edge within the mortar, from whence the coarfer particles again fall into the groove of the muller, and are again ground under it; which operation is continued, till the whole of it is ground to an impalpable powder; the muller is then eafily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is ufually placed upon the mortar, during the operation, to prevent any lofs to the colour, or bad effect to the operator.

A new

Mr. Murray's Portable Steam Engine.

Four Horse power.
Scale ¾ Inch equal 1 Foot

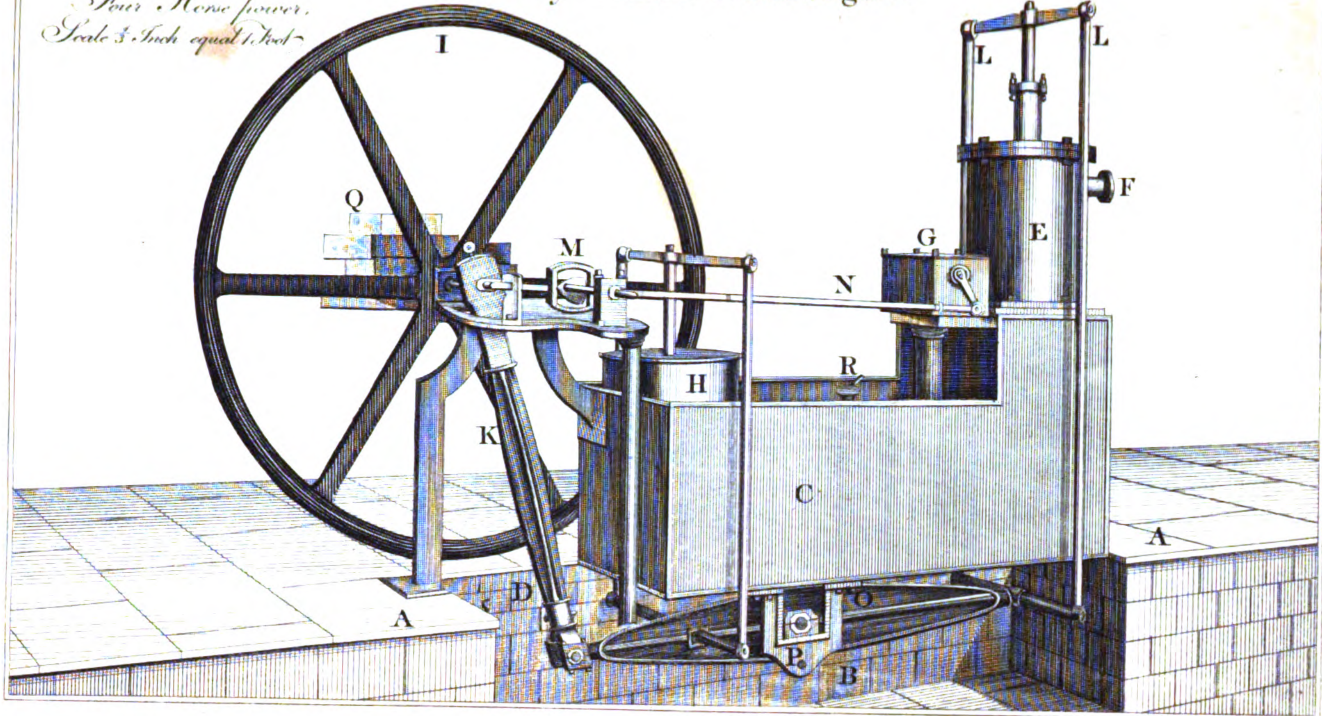
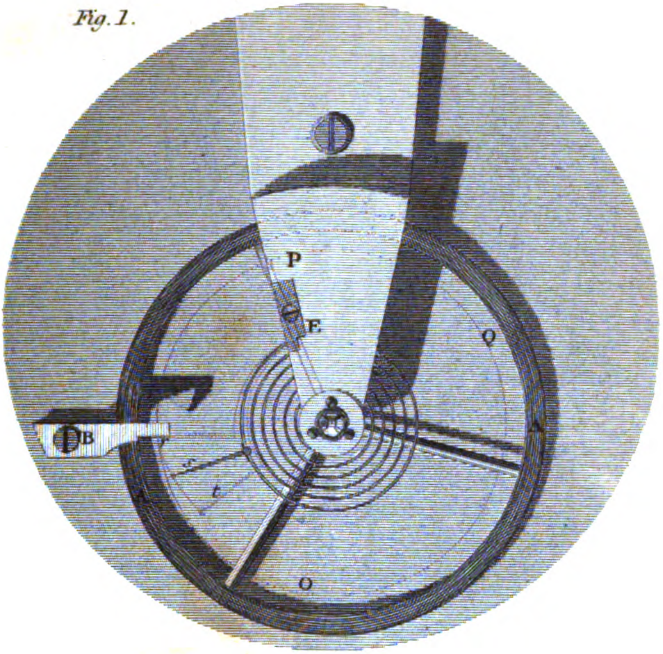
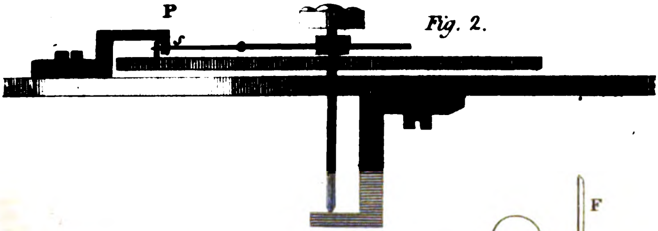


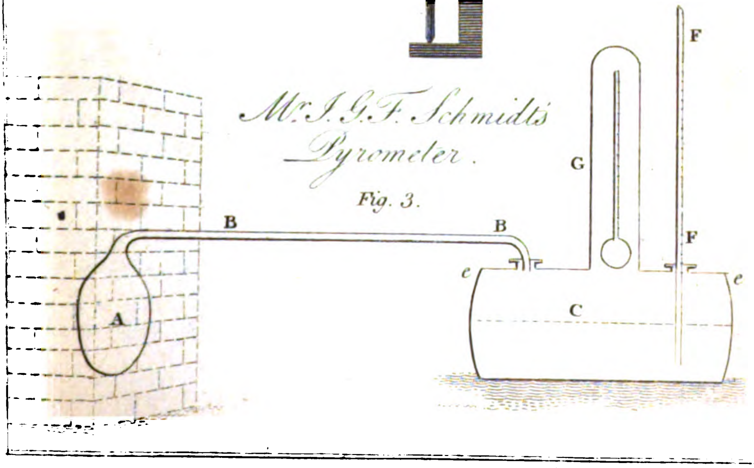
Fig. 1.



Mr. Hardy's method of banking the balance of a Time keeper.

Fig. 2.





Mr. I. G. F. Schmidt's Pyrometer.

Fig. 3.

XVIII.

A new and cheap Method of purifying Gold and Silver. By ANDREW THOMSON, *Esq. of Banchory, near Aberdeen. In a Letter from the Discoverer.*

To Mr. NICHOLSON.

SIR,

I INTENDED to have deferred the present communication till such time as I should have it in my power to lay before the public the complete series of experiments in which I have been engaged with regard to the purification of gold and silver. But unluckily I mentioned a few particular circumstances with regard to them, to a man who took it upon him, without my knowledge, to send an account of them for publication to a periodical work. As I understand that work will not appear so soon as your next number, I beg, if you think it worthy of a place, that you will insert the following account of some attempts I have been making to purify the precious metals.

This publication is prematurely made, because the inventor might else be anticipated.

Being much at a loss for want of a crucible of pure silver for the analysis of some minerals, and as all the usual methods practiced for purifying that metal are very troublesome, I set myself to consider the various operations on metals, in hopes of falling on a more simple way of accomplishing my purpose. At length, I found a process of Pelletier's, which promised to succeed, and mine is merely extending his idea a little further than he did himself.

Process of Pelletier for extracting copper from bell-metal.

He was, I believe, employed by the French government to discover an easy way of separating the tin from copper on bell-metal, and the process he gave, is this. Upon the melted bell-metal project black oxide of manganese in powder, frequently stirring the metal till all the tin becomes oxidated by the manganese. He adds a caution, not to add too much manganese, otherwise part of the copper also will be destroyed.

He oxides the tin by manganese; in such proportion as not to affect the copper.

It immediately struck me, that in this way I might be able to oxidate the copper which alloys our silver, and upon making the trial I succeeded completely; I had some impure silver rolled out to about the thickness of a shilling, this I coiled

The author's improved process. Coarse silver was rolled out; then coiled up; bedded in manganese; and

Strongly ignited for a quarter of an hour. All the metal was oxided.

coiled up spirally, and put into a crucible, the bottom of which was covered with black oxide of mangaNese. I then added more oxide till the silver was covered, and all the space between the coils completely filled. A cover was then luted to the crucible, and a small hole left for the escape of oxigen gas. When this had been exposed for a quarter of an hour to a heat sufficient to melt silver, I found the surface of the manganese brown from the loss of oxigen; but, where the silver had been, the whole was one uniform black powder, without the least appearance of metallic lustre, so that I had no doubt, that even the silver was become an oxide.

The whole contents were then put into a larger crucible with thrice its bulk of green glass.

I then put the whole contents of the first crucible into a second of a larger size, into the bottom of which I put a quantity of pounded green glass, about three times the bulk of the contents of the first crucible, and luted on a cover as before, to prevent the access of any inflammable substance.

Strong heat fused the glass, and reduced the silver pure, and alone in a button.

The crucible was then exposed to a heat sufficiently strong to melt the glass very fluid. Upon cooling and breaking the crucible, I found the silver at the bottom perfectly pure, as its oxide alone could part from its oxigen without the access of some inflammable substance. I find this process answers equally well for purifying gold, and to me it seems to possess some advantages over all the former methods. The materials used are cheap, and a large quantity can be refined as soon, and as easily as a small quantity, by merely altering the capacity of the crucible you use.

This process answers equally with gold.

The metal must be in thin or small masses.

I tried the same operation on gold and silver in round masses, but found it went on very slowly, and what I scarcely expected, in the first part of the process of oxidating the metals, the remaining metal continued uniformly impure or nearly so, until the whole was oxidated.

The proportions, &c. are not here given, because the author has hastened to communicate his process.

I regret that I have been forced to make this matter public, before I could do it in a manner satisfactory to myself. I wished to have given the exact proportions of alloy, manganese, and glass to be generally used, and to have ascertained if there is any truth in the old opinion, that saltpetre melted with gold destroys a part of it. I suppose that idea may have arizen from the oxigen given out by the nitre in a high heat, oxigenating the copper contained in the impure gold, which has been the subject of the experiment.

 Since

Since the above was written, I have been informed that this matter has actually been published, but know not in what work. I hope you will still have the goodness to insert this as an *original communication*, as I do not think the person who has published it will have the impudence to call it his own, and as Mr. Kirwan, and other celebrated chemists long ago advised me to publish it, I have already stated my reasons for not following such good advice. He vindicates his claim.

As I have now been forced to appear before the public, I have hopes I shall be able to prevail on some of my friends to commit themselves in the same way, in the confidence that their labours will be found useful to the public.

I am, Sir,

Your's truly,

ANDREW THOMSON.

Banchory, by Aberdeen,
May 5th, 1805.

XIX.

Memoir on the Propagation of Sound. By M. HASSENFRATZ.*

THE production of sound is ascribed by all natural philosophers to the vibration of the molecules of bodies. Sound produced by vibrations,

The vibration of these molecules admits of two kinds of modification; 1st, in velocity; 2ly, in magnitude. The first of these determines the nature of tones; the second, their force or intensity. which differ in velocity and in magnitude.

Sound is transmitted to the ear by the molecules that fill the medium or interval between the sonorous body and the organ of the hearing. The movement of the sonorous body impresses on the molecules of the medium an impulse, which they transmit from one to another, till it reaches the ear, with a greater or less velocity. In this transmission the vibration may undergo two kinds of alteration: 1st, in its velocity; 2dly, in its intensity. In this memoir I shall only transcribe some experiments relative to the velocity of sound. The sonorous body impresses its vibrations on some medium, the vibrations of which are continued to the ear.

Philosophers have long been engaged in determining the velocity of sound, but considering the air as the chief medium by The velocity of sound hitherto examined only in air.

* Annales de Chimie, Vol. LIII. p. 64.

by which it is transmitted to the ear, they have attended only to its velocity in air, and have employed two different methods to determine this, theory and experiment.

This velocity uniform in a given medium,

and the density of the medium is one of its elements.

These two methods have led to the following remarkable results: 1st, that the velocity of sound in a given medium, is uniform, whatever its distance from the phonic centre, and whatever its intensity: 2dly, that the density of the medium at equal pressures is one of the elements of the velocity; for it has been found by theory, that the velocity of sound is the same as that of a body falling from half the height of an atmosphere supposed to be of equal density with the air in the place where the sound is transmitted; and by experiment, that, all other circumstances being equal, the velocity of sound is the same at different pressures of the barometer; so that it is equal on the summit of a mountain and on the sea-shore. In fact, the density of the air being proportional to the compressing weight, the height of the column of mercury in the barometer, divided by the density occasioned by this pressure, is a constant quantity; and the height of the atmosphere of a uniform density being equal to the total weight of the air divided by its density, it follows that the height of the barometer ought to make no difference in the velocity of sound. Blanconi asserts (Comment. Bonon, vol. II. p. 365), that the velocity of sound is less in winter than in summer, since, according to his experiments, it takes four seconds more in winter to traverse a space of sixteen Italian miles. Derham affirms, that the velocity of sound is the same whether the air be extremely hot or extremely cold, though his tables of experiments will be found on examination favourable to the opinion of Blanconi; for the greatest velocity of sound in them was on the 5th of April, at one o'clock in the afternoon, being three miles in 111 half-seconds, and the least velocity on the 12th of February, at six o'clock in the evening, being three miles in 122 half-seconds. As the experiments on the velocity of sound undertaken by the Academy of Sciences in 1737 were made at temperatures that exhibit only two or three degrees difference, perhaps it would be well, as Mr. Laplace thinks, if they were repeated at a time of the year when the temperature is very different; for experience has taught us, that this velocity is equal in rainy and in fine weather, so that nothing but change of temperature can produce any variation in this respect.

Not affected by the height of the barometer.

Blanconi asserts it to be less in winter than in summer.

Derham denies this, but is probably wrong.

Not affected by rain or fine weather.

Wha

4

Whatever these results may prove, as the experiments on the velocity and propagation of sound have been hitherto made in the air alone, it was an interesting enquiry to determine the velocity of sound, when transmitted by other bodies, and particularly of different densities with respect to air. Mr. Laplace, to whom branches of physical science are indebted for improvement, invited me about eight months ago to make experiments on this subject, and particularly on the propagation of sound through solid bodies; and the experiments of which I shall give an account in this paper were principally made in consequence of that gentleman's suggestion.

Making experiments in the quarries beneath Paris on the transmission of sound through long galleries, I caused a person to strike with a hammer against a mass of stone, retiring at the same time by degrees from the place of striking, in order to distinguish if possible the sound transmitted by the stone from that transmitted through the air. Placing my ear against the mass of calcareous stone through which these galleries are carried, at a short distance I distinguished two sounds perfectly separate, one transmitted by the air, the other by the stone. Both sounds grew weaker in proportion as I retired from the striking point; but that transmitted by the stone was weakened much more rapidly than that transmitted through the air. In a gallery excavated beneath Rue de la Harpe the sound transmitted by the stone ceased to be audible at 134 paces distance; and in a gallery beneath Rue de St. Jaques at 140 paces.—— Through the air the sound was transmitted to 400 or 440 paces distance. The sound transmitted by the stone always reached the ear much sooner than that transmitted by the air.

Experiments on the transmission of sound through the stone in quarries.

It was conveyed through the stone only 140 paces; —but through the air 440. It passes quickest through the stone.

Mr. Berthollet, to whom Mr. Laplace imparted these results, desirous of being assured whether the sound of a hammer could be transmitted through a mass of stone 140 paces thick, requested Mr. Gay, by my desire, to be present at my experiments. With this young chemist I repeated the experiment of the transmission of sound through stone on several separate masses, and he convinced himself, that sound was capable of being transmitted through a mass 150 paces in length.

Experiment repeated, and the sound conveyed 150 paces.

It was long ago observed in working mines, that the noise was propagated to a very great distance through masses of rock; and the line of the sound heard through the stone serves on many occasions to determine the direction in which the gal-

The propagation of sound through stone long ago observed by mechanics.

ſeries are carried on; but no one had attempted to obſerve, whether the velocity of the ſound tranſmitted by the ſtone differed from that of the ſound tranſmitted by the air. My experiments in the quarries underneath Paris have taught me, that the difference is conſiderable; and when the gallery is ſufficiently ſtraight, to be able to diſcern the motion of the hammer with the eye, no calculable difference can be perceived between the conveyance of the motion to the eye and that of the ſound to the ear.

Its velocity apparently equal to that of light.

The diſtance which the ſound of the hammer can be conveyed to the ear varies conſiderably with the nature of the ſtone and the ſeparations or fiſſures in the maſs. Having cauſed a man to ſtrike with reiterated blows againſt an iſolated wall, built of common ſtone of the ſame kind as that in the quarries, and cemented with mortar, the ſound was tranſmitted only thirty paces. Striking in the ſame manner on a parapet of hewn ſtone raiſed on the borders of the Seine, the ſound was tranſmitted 46 paces. Theſe experiments were made in the open air by day, conſequently under circumſtances leſs favourable to the propagation of ſound than when on the calcareous maſſes in the quarries; but the difference between 30 and 46 paces, under the ſame circumſtances, on maſſes differing only in the dimenſions of the ſtones of which they were formed, is very remarkable.

The differences of the diſtances to which ſound is tranſmitted through ſolid maſſes remarkable.

Encouraged by the ſucceſs of my experiments in the quarries, and by the invitation of Mr. Laplace, I attempted to repeat the ſame experiments on different ſubſtances.

Experiments repeated,

on timber.

By the ſide of the high road that leads from the place *de la Concorde* to *Chaillot* along the bank of the Seine, on the ſtone wharf of St. Leir, oppoſite the ſteam-engine of Gros-Caillou, is placed a railing 210 paces in length, formed of 31 pieces of timber, ſeparated by four large poſts. The blow of a hammer at one extremity of this railing was heard diſtinctly at the other, though through the air it was audible only 120 paces. At the diſtance at which both the ſounds were audible, that through the wood was heard long before the other; and when, ſtanding at the greateſt diſtance from the place of the blow, I heard only the ſound tranſmitted through the timber, the velocity of its tranſmiſſion was ſo great, that it was difficult to diſtinguiſh any interval between the perception of the ſound by the ear, and of the motion of the hammer by the eye.

Conveyed farther than in the open air.

Its velocity apparently equal to that of light.

Having

Having convinced myself, that the propagation of ſound through ſtone and through wood was effected with much greater velocity than through air, and that the time of its tranſmiſſion to ſuch ſhort diſtances as thoſe on which I was able to make experiments was too little for calculation, I was deſirous of knowing whether the velocity of its tranſmiſſion through metallic ſubſtances were the ſame.

Several experiments on bars of iron fixed on ſolid maſſes, as the bars that hold together the ſtones of parapets, having given me uncertain reſults, I ſought for iſolated bars of ſufficient length to afford ſome certainty. My firſt experiment was made on the upper bars of an iron railing, 34 paces long, erected on one of the walls of the garden of the Legiſlative Body, adjoining to the Place des Invalides. On ſtriking one extremity of this aſſemblage of bars, two diſtinct ſounds were heard at the other end; that tranſmitted by the bars, and that by the air; the former being always heard firſt. The ſame experiment afterwards repeated on bars of different lengths, gave me the ſame reſult; and this reſult is ſuch, that it is impoſſible to diſtinguiſh, at the ſmall diſtances at which theſe experiments were made, any difference between the tranſmiſſion of the motion by light, and that of the ſound by the ſolid medium.

Similar experiments and reſults with long bars of iron.

Repeating my experiments on the velocity and propagation of ſound through the maſſes of ſtone in the quarries beneath Paris, in company with Mr. Gay, this young chemiſt imagined that he diſtinguiſhed two ſounds tranſmitted through the air, one grave and the other acute, which reached his ear in ſucceſſion, the graver ſound appearing to have the greater velocity.

Mr. Gay ſuppoſed he heard two ſounds through the air, firſt a grave, then an acute.

This reſult, though contrary to the theory of the propagation of ſound, according to which grave and acute ſounds ought to have the ſame velocity, had already been conjectured by ſeveral philoſophers, particularly by Mairan, and was therefore worth confirmation. For this purpoſe I ſtretched two ſtrings, one of braſs, the other of catgut, ſo as to make them emit two different tones, the firſt one more acute, the ſecond one more grave; then ſtriking both theſe ſtrings at once with the wood of a black lead pencil, the two ſounds, which were confounded together at firſt, appeared to ſeparate at the diſtance of 400 paces in a large gallery of the quarries, and we both imagined we could diſtinguiſh the graver ſound firſt.

Mairan and others had conjectured the ſame.

Experiment to determine this.

While

Probable cause of fallacy in this experiment.

While observing the propagation of sound in the galleries, I had several times occasion to remark, that a sound at a great distance was frequently repeated, either by reflection or by the vibration of the walls, so as to cause two differerent sounds to be heard and distinguished, which reached the ear in succession. As it was possible that the difference which appeared to Mr. Gay and me might have been produced by the cause here mentioned, I determined to repeat the experiment in the open air.

The experiment repeated in the open air.

For this purpose I took two glass bells, the tones of which were as an octave and a fifth, that is, the ratio of their vibration was as 1 to 3. A hammer was so adjusted as to strike both the bells at once, and make them sound at the same instant. Carrying this instrument into the fields, I endeavoured to ascertain whether the two sounds reached the ear in unequal times. Several experiments repeated in various places, made me believe a long time, that their velocity was unequal; but having observed, that on some occasions the sounds reached the ear at the same time, I was led to remark, that whenever I imagined I distinguished a difference of velocity, this difference had been occasioned by a repetition of the sound, and that frequently very trifling obstacles, as trees or hedges, were sufficient to produce this repetition. I repeated my experiments therefore anew in the midst of plains of greater or less extent, as those of Montronge, Grenelle, St. Denis, &c. and whenever I was remote from any obstacle capable of producing a repetition, both sounds were heard at the same time. With my instrument I could distinguish the sound of the two bells at the distance of 700 paces or 631 yards; whence it follows, that both experiment and theory concur to demonstrate, that grave and acute sounds have the same velocity.

The double sound produced even here by an echo.

Grave and acute sounds have the same velocity.

Conclusions from the experiments.

From the experiments related in this paper it follows: 1st, that the velocity of sound differs according to the medium by which it is propagated: 2dly, that this velocity is much more considerable, when it is propagated by solid and very dense bodies, than when by aeriform bodies, and of little density: 3dly, that both grave and acute sounds have the same velocity; a result to which theory led us.

A very

XX.

A very advantageous Mode of preparing Muriate of Barytes by the Mutual Decomposition of sulphurated Barytes and Muriate of Lime. By Mr. GOETTLING.*

MR. DRIESSEN of Groningen first observed, that muriate of lime and sulphate of barytes decompose each other at a high degree of heat. Trommsdorff verified the experiment of the learned Dutch professor, and applied the principle to the preparation of muriate of barytes. He perceived, however, that the whole of the sulphate of barytes was not decomposed in this way, and he thought, that equal parts of the two salts were the suitable proportion for decomposing them as completely as possible. Mr. Goettling has deemed the subject worth farther inquiry, the result of which was the process I shall now describe.

Driessen first decomposed sulphate of barytes by muriate of lime; and next Trommsdorff.

One part of native sulphate of barytes in fine powder is to be mixed with half a part of muriate of lime.† This mixture is to be introduced into a Hessian crucible, which is to be closely covered, and brought gradually to a red heat. The matter must be kept in a state of incandescence a full half-hour, and frequently stirred. It is then to be poured out into an iron cone, and after being coarsely powdered, is to be thrown into three parts of boiling water. The vessel being immediately taken from the fire, the mixture is to be stirred occasionally with a glass spatula, and the undissolved matter is then to be left to subside. The clear liquor being decanted off, the residuum is to be poured into a filter of linen of a close texture, and the fluid lightly pressed out. The residuum being again lixiviated with one part of water, is to be strained as before. The liquors are then to be mixed together, and evaporated to a pellicle, to obtain the salt by crystallization. In this way we shall have five-eights of a part of muriate of barytes. The

Mr. Goetling's process for preparing muriate of barytes in this way.

* Van Mons's *Journal de Chimie*, Vol. VI. p. 92. Abridged from the *Taschen-Buch fuer Scheidekuenstler*.

† This muriate is obtained in abundance in our laboratories, as an adventitious product: it may be procured likewise at a very trifling expence by adding lime to the mother-water left after refining common salt.

Muriate of lime, how obtained.

mother-

mother-water of the firſt cryſtallization, which is almoſt wholly muriate of lime, is to be ſet aſide for a freſh operation, or for any other purpoſe.

As the inſoluble maſs of ſulphate of lime ſtill contains a large quantity of ſulphate of barytes, indeed about half the original quantity, it is to be treated afreſh as above, with one fourth part of muriate of lime; and the ſame proceſs is to be repeated with an eighth part of the ſame ſalt. Thus we obtain an addition from one to two eighth parts of muriate of barytes. The ſalts of the various cryſtallizations require to be diſſolved and re-cryſtallized anew, in order to free them from a little muriate of lime, which adheres to them in the firſt cryſtallization.

The ſalt to be purified by re-cryſtallization.

XXI.

Obſervations on the Rectification of Nitric Acid, by Mr. STEINACHER, *Druggiſt at Paris.**

Nitric acid contaminated in its firſt portions with muriatic. Why, according to Berthollet.

IT has long been known, that the firſt portions of nitric acid diſtilled from litharge contain muriatic acid. Berthollet explains this phenomenon by ſaying, that the oxide of lead dividing its action between the two acids, both are ſubjected to the action of expanſibility produced by the heat. Meſſrs. Welter and Bonjour aſſert, that, if muriate of ſilver be employed, oxigenated muriatic acid is formed, which riſes with the firſt portions. If I may be allowed to give the reſults of my labours after thoſe of ſo many able chemiſts, I would ſay, that, when the nitric acid has been ſufficiently concentrated before being ſubjected to rectification on ſilver, or on oxide of lead, the firſt part of the rectified acid is found on trial to contain no muriatic acid, notwithſtanding the nitric acid contained much of it after its concentration; and hence I infer, that an exceſs of water is the true cauſe, that diminiſhes the attraction of the muriatic acid for the oxide of lead or of ſilver.

True reaſon is the preſence of an exceſs of water.

The operator however would in vain expect to ſucceed by merely concentrating his acid before rectifying it, if he uſed a determinate proportion of litharge, or diſtilled to dryneſs, as ſeveral

Concentrating the acid inſufficient, if the proportion of litharge be improper, and the diſtillation carried to dryneſs.

* Van Mons's *Journal de Chimie*, Vol. VI. p. 88.

feveral authors direct. The quantity of litharge muft vary from one to eight fixteenths of the weight of the acid, according to its degree of impurity. On the other hand, if diftilled to drynefs, the latter portions of the nitric acid will carry over with them in folution muriate of lead, or of filver.

Mr. Steinacher's procefs.

Four kilogrammes (10lb. 8oz. 16dwt. troy) of nitric acid of the fhops, at 35°, containing muriatic acid, and a very little fulphuric, are firft diftilled in a reverberatory furnace in a retort placed on an earthen veffel filled with fand. The fire muft be fo regulated that the drops fucceed each other flowly, and half the acid is to be thus drawn off. It will then give 15° of Baumé's areometer. What remains in the retort is to be poured into a bottle. Its fpecific gravity will be expreffed by 4° of the areometer. Litharge being thrown into it in fine powder, and ftirred with a glafs rod, will be converted into a white powder in a few hours. More litharge is then to be added in the fame manner; and this is to be continued, till the litharge retains its colour after feveral hours ftanding. The muriate and fulphate of lead are then to be left to fubfide entirely, and the acid is to be decanted off into a tubulated glafs retort, placed on a fmall earthen plate filled with fand, in the midft of a reverberatory furnace, all the parts of which are retained except the dome. A receiver is to be adapted, which fits clofely without luting; for, as the vapour of the acid eafily deftroys every kind of lute, the product would otherwife be liable to become impure; and the diftillation is to be fo conducted, as to admit a fhort interval between the fall of each drop. Great care muft be taken not to fuffer the acid to boil, for thus it would be diffipated in incompreffible vapour. The firft half that comes over marks 35°, the fecond 40°. Both portions are colourlefs, and have all the properties of a very pure nitric acid, if $\frac{1}{12}$ of the liquor be left in the retort.

Beautiful cryftals of muriate of lead may be obtained when half the acid is diftilled off.

If a ftop be put to the diftillation after the firft portion is feparated, and the retort left to grow cold, you will obtain a beautiful cryftallization of muriate of lead in large and very brilliant ftriated hexaedral laminæ. This falt is a true muriate, for fulphuric acid expels from it vapours eafily diftinguifhable to be thofe of the muriatic acid. On continuing the diftillation, thefe cryftals gradually lofe their regular figure, and at length fall to the bottom in a powdery precipitate.

Eafy

XXII.

Easy Method of making the very combustible Oxide of Phosphorus. *By* AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Oxide of phosphorus.

YOU know very well that phosphorus united to a much smaller proportion of oxigen than is requisite to render it into the acid state, brings it into the condition of an oxide which sets on fire sulphur, on just rubbing it against a common match. But the common method of oxidizing phosphorus for phosphoric matches by fixing it in the bottles with a hot iron, is troublesome and wasteful. This oxidation, however, may be effected with great facility and economy by exposing a large proportion, viz. a hundred grains of phosphorus in a jar containing half a pint measure of oxy-muriatic acid, in which circumstance the phosphorus will be melted and fume, but scarcely take fire. After cooling, it must be kept excluded from the air, to prevent the inflammation from mere exposure.

DEAR SIR, Yours,

AMICUS.

May 28th, 1805.

XXIII.

Description of an extremely sensible Micro-electrometer. *By Mr.* MARECHAUX.†

A piece of leaf silver is suspended so as to be moveable in a glass cylinder.

IN a glass cylinder, about an inch and half in diameter and five or six inches high; a piece of leaf silver is suspended from a small pair of nippers, capable of being lowered or elevated as the length of the leaf may require. The piece that carries the nippers may likewise be moved horizontally, so that the leaf may be moved at pleasure nearer to or further from a sphere of copper, which is one of the poles of the instrument.

* Translated from Von Mons's *Journal de Chimie,* Vol. VI. p. 38. Abridged by Van Mons from Gilbert's *Annalen der Physik.*

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The glaſs cylinder has, about 1″ (centimetre, near 4 lines Engliſh meaſure?) from the plate on which it is fixed, a ſmall hole, through which paſſes the extremity of a micrometer-ſcrew, about the ſize of a large gooſe-quill, and very carefully cut. This ſcrew has fifty threads in a Rhynland inch, cut very deep, though very fine. It is made of two pieces, and ſhould be at leaſt three quarters of an inch long, to avoid any ſhake. The extremity of this ſcrew carries a little ball, which is put on after the ſcrew is paſſed through the opening in the cylinder. To avoid all friction againſt the glaſs, care is taken that the ſcrew, when turned, does not touch the edges of the cylinder. The ſcrew carries a plate three inches and a half (3.8 Engliſh) in diameter, which has 360 diviſions, and conſequently divides each thread of the ſcrew into as many parts.

Through a hole in the cylinder a micrometer-ſcrew paſſes,

In this manner we are enabled to determine the ſphere of activity of the two electricities in 18000ths of a Rhynland inch. The mounting which contains the female ſcrew has a ſmall pillar, which advances on the plate, and carries an index, by which the degrees are marked with preciſion.

by which the diſtance of $\frac{1}{18000}$ of an inch may be meaſured.

To uſe this inſtrument, which is perfectly inſulated by the glaſs plate on which it reſts, the firſt thing is to place the plate in ſuch a poſition that the 0 ſhall be exactly under the index. The adjuſting ſcrew which carries the nippers is then to be moved till the leaf ſilver is ſo near the ball, that no light paſſes between them. Thus we have the point of contact, and of 0 for the ſphere of activity of the two electricities. To be certain that the leaf is brought as cloſe as poſſible to the ball without being forced out of the vertical direction, the micrometer ſcrew ſhould be moved a turn firſt backward then forward ſeveral times, and the poſition of the leaf obſerved every time the ball is brought into contact with it. The inſtrument being thus adjuſted, the micrometer-ſcrew muſt be moved backward one turn, and then we have between the leaf and the ball a diſtance of one-fiftieth of an inch, which may be ſubdivided at pleaſure by means of the plate; for with a plate near four inches in diameter, and by means of the fine needle on which it turns, we may diſtinguiſh half or even a quarter of a degree if neceſſary.

Mode of preparing the inſtrument for uſe.

An adjuſtment fixed to the plate on which the cylinder reſts ſerves to aſcertain whether the leaf of ſilver be in fact drawn out

Apparatus for aſcertaining the perpendicularity of the leaf.

ont of the vertical line by any attractive power. This consists of a fine silk thread, stretched in the same plane as the silver leaf. By means of the screw this thread may be moved both horizontally and vertically, so as to follow the movements of the silver leaf.

Mode of using the instrument.

Every thing being thus arranged, make a communication by means of conducting wires between a single couple of metallic disks, or one constituent part of a pile, placed on a plate of glass, and the instrument, so that one of the metals shall communicate with the top of the instrument, and the other with the bottom. Then by means of a glass handle fitted to it move the plate slowly from one degree to another, and you will find the leaf touch the ball with ordinary electricity, when it is 60° or 80° of the micrometer screw from the vertical plane, in which the leaf silver rested before its communication with the metallic disks. This distance increases for every pair of disks added; and as the ball remains fixed at the point to which the screw has carried it, the motion of the instrument may be observed with great accuracy.

Its extreme sensibility.

This instrument is so sensible, that, if a slender glass tube be rubbed but twice, and brought near the apparatus, though several inches from its summit, it passes through the whole extent of its scale. It is for this reason the inventor calls it a *micro-electrometer*, because we can measure only very weak degrees of electricity with it.

XXIV.

Action of Phosphorus on the Solutions of Metals. By Mr. SCHNAUBERT *.

Phosphorus observed to precipitate metals, by Sage, Ilsemann, and Mrs. Fulhame.

SAGE † had already observed, that phosphorus precipitates the sulphates of copper and of manganese. After him Ilsemann ‡ obtained a crystallization of silver in the humid way by means of phophorus. Still more recently Mrs. Ful-

* Van Mons's *Journal de Chimie*, Vol. VI. p. 95. Abridged from Goettling's *Chemisches Taschenbuch*.

† *Analyse chimique et Concordance des trois Régnes.*

‡ Crell's *Chemische Annalen*, 1789, Tom. II. p. 323.

2 hame

hame* published experiments on the reduction of some metals by phosphorus dissolved in ether. Lastly, Mr. Schnaubert has undertaken a new investigation of the subject, the principal results of which are as follows:

Schnaubert's experiments.

Gold.

Two little bits of phosphorus were put into a nitro-muriatic solution of gold diluted with a small portion of water. At the expiration of twenty-four hours the solution was completely colourless, and pellicles of the colour of metallic gold swam on the surface of the liquid. The phosphorus itself was covered with a deep brown coating, and in this was observable in several places thin layers of reduced gold. At the place where the phosphorus was a black circle was perceived. The solution thus treated by phosphorus, had not a single atom of gold left in it.

Gold completely precipitated in a metallic form by phosphorus.

Silver.

Some phosphorus, which was left for twenty-four hours in a nitric solution of silver diluted by water, was completely covered with metallic silver in the form of dendrites, the ramifications of which were directed upwards. During ebullition this remarkable crystallization of silver, which made the phosphorus appear as if garnished with points, assumed first a white colour, and afterward formed a light black mass, which at length became of a light brown colour.

Silver precipitated in dendritic crystals,

which boiling converted into a phosphure.

Quicksilver.

Mercury dissolved in nitric acid is precipitated on the phosphorus in the form of little metallic globules, which cover it entirely. By heating to ebullition the mercurial globules disappear, and a black mass without any metallic lustre is formed.

Mercury precipitated in globules,

which heat converts to a phosphure.

Lead.

At the ordinary temperature phosphorus did not act on the nitric solution of lead, though the digestion was continued for several days: at a boiling heat however a change was observed in the phosphorus, which was covered with a grey colour slightly metallic.

Lead not reduced without a boiling heat.

* Essay on Combustion, &c.

Copper.

Copper.

Copper powerfully acted on.

No metallic solution was more strongly attacked by phosphorus than that of copper in nitric acid. The phosphorus was no sooner introduced into the solution of copper, than it assumed a black colour. In twenty-four hours it was covered with a stratum of metallic copper in very thin layers *; and the solution had become much paler. The application of heat caused drops of phosphorus to flow out upon the reduced copper, where they immediately assumed a black colour. These drops after a time were in their turn covered with a metallic coat of copper. After this the solution was perfectly colourless, and ammonia did not detect in it the least particle of metal.

Completely precipitated from the solution.

Tin.

Tin partly reduced, partly converted into phosphure.

Several bits of phosphorus were put into a solution of tin in nitro muriatic acid. The next day the phosphorus was coloured of a deep brown, only in some parts a metallic colour was observable. These metallic spots disappeared on boiling, and the phosphorus became still deeper coloured.

Sulphate of copper forms a beautiful experiment.

* In making this experiment with a solution of sulphate of copper, and slightly heating the mixture, at first a vapour arises, consisting of phosphorus gas, that carries off with it some small particles of phosphorus, which take fire on the surface of the solution: but the extrication of this vapour soon ceases, and the phosphorus becomes hermetically enclosed in a box of copper, in which it is defended against any farther action of the sulphate, and even of the air, to whatever temperature short of fusing the copper it be afterwards exposed. The plate of copper that forms this covering is two or three lines thick: it possesses more tenacity than common copper, for it may be flattened with a hammer in different directions without cracking, which at the same time proves the great compressibility of the phosphorus; and it shines with a very pure metallic lustre. On opening the box carefully with a cutting instrument, the phosphorus is found in it retaining perfectly its form, filling its copper case completely, and not appearing even to have acted upon the sulphate.

The phosphorus enveloped in a case of particularly malleable copper impervious to air.

Other metals did not produce this effect.

I did not obtain the same effect with several other metals which I tried, no doubt on account of their containing more oxigen.

VAN MONS.

Manganese.

Manganese.

Some bits of phosphorus were put into a sulphuric solution of manganese. The next day the phosphorus was of a deep brown colour, and on its surface was perceivable a pleasing mixture of colours, owing to the reduced metal. The mixture was then heated to ebullition, and after it had grown cold, the manganese was found reduced in the form of little radiating lines on the fused phosphorus, intermixed with a few small globules of the white colour of tin.

Manganese reduced by it.

In these experiments we may observe, that the phosphorus, beside deoxidating the metals, united with the metals when reduced to form phosphures, as was evident in the solutions of silver, mercury, and tin.

General conclusions.

XXV.

Account of a new Pyrometer, which is capable of indicating Degrees of Heat of a Furnace. By Mr. J. G. SCHMIDT, *of Yassy, in Moldavia. From the Author.*

WITHOUT entering into any detail concerning the substances best calculated for pyrometrical enquiries, I flatter myself that it will be admitted that those must receive the preference which are capable of regularly contracting or expanding, without altering their chemical properties, when subjected to elevated temperatures.

Pyrometrical substances.

The permanently elastic aeriform fluids appear to me to be superior in those respects to any other bodies.

Gases are the best for strong heat.

Let atmospheric air be freed from moisture by caustic alkalis, or other bodies, and included in a vessel of platina. This vessel A *(Fig. 3, Pl. VI.)*, which may be made of any convenient size, is connected with the tube B B, of as fine a bore as possible. This tube is also made of platina, and reaches into a vessel C, which is filled with water up to *e e*, and into this the tube is fixed air-tight. Out of the vessel C rises a glass cylinder G hermetically sealed, including a thermometer, and a graduated tube F F is secured into the vessel C in a similar manner.

Atmospheric air in a vessel of platina.

This

Use and application. This is the whole construction of my pyrometer. To make use of it nothing more is necessary than to introduce the platina vessel A into the furnace the temperature of which is to be learned. The moment the included air is acted upon by the heat it expands, and expels the water up into the graduated tube F F. This rise will take place accordingly as heat increases. If care be taken that the air be cooled in the vessel C as much as possible (which will be the case from the large surface of water to which it is exposed), it is obvious that a volume of water equal to the volume of air in the vessels of platina, can never pass up into the tube. The refrigeration may be facilitated by the application of vaporizable fluids, such as ether, alcohol, &c.

If the degree of temperature be obtained which the air had before it was subjected to the experiment, and a proper allowance be made for the pressure of the water in F F, the true expansion of the air may thus be found, and compared with the respective temperatures.

XXVI.

New, easy, and economical Method of separating Copper from Silver. By *Mr.* GOETTLING *.

Sulphuric acid used instead of nitric on account of cheapness, THERE are four methods of separating copper from silver, all of which require the alloy to be dissolved in nitric acid. As this acid is very dear, Mr. Geottling thought of using the sulphuric in its stead, which is comparatively very cheap. His and with perfect success. success perfectly equalled his expectation, and the following is his method:

Description of the process. Having ascertained by the touchstone, or in any other way, the proportion of silver contained in the alloy, take one part of sulphuric acid for every part of silver, and for every part of copper three parts and three-fifths of a part of the same acid. Dilute the acid with half its weight of water, and pour it into a matrass on the alloy reduced to very small pieces. In order

* Translated from Van Mons's *Journal de Chimie*, Vol. VI. p. 77. Originally published in the *Taschen-Buch fuer Scheidekuenstler*, and abridged by Van Mons.

to promote the action of the acid, it is of use to put one part more to every sixteen parts of the alloy. The matrass is then to be placed in a sand-heat, and the acid brought to a state of ebullition. In two or three hours time the alloy is commonly dissunited and converted into sulphate, particularly if care be taken to stir the mass from time to time with a glass spatula. This mass is thick, and frequently hard. While it is still hot, six or eight times its weight of boiling water is to be added to it, and it is to be left some time longer on the fire. The sulphate of copper will be dissolved, and great part of the sulphate of silver will be precipitated. The operator will now examine whether the whole be completely dissolved; and if it be, a plate of copper, or some pieces of copper or halfpence tied up loosely in a piece of coarse linen, must be suspended in the mixture, and the whole kept boiling for some hours. The sulphate of silver will thus be decomposed, and the silver separated in the metallic state.

To ascertain whether the separation be complete, a few drops of solution of muriate of soda are to be dropped into a little of the liquor. If a cheeselike precipitate be formed, it is a proof, that all the silver is not separated, and in this case the ebullition with the copper must be continued longer. After the whole of the silver is separated, the liquor is to be poured off, the precipitated silver is to be well washed, and the entire separation of the cuprous salt is to be ascertained by the addition of a few drops of liquid ammonia to the water with which the precipitate has been washed, which, if it contain any copper, will be rendered blue by the ammonia. After the silver is thoroughly freed from the sulphate of copper, it may be kept in the state of powder as it is, or it may be fused with a fourth or at most half its weight of sulphate of potash.

Mode of ascertaining whether the separation be complete.

May be kept in powder, or fused with sulphate of potash.

The water poured off is then to be mixed with what was used for washing the precipitate, and evaporated in a copper pan, so as to obtain the sulphate of copper by crystallization. The blue vitriol thus produced will be at least equal in value to the sulphuric acid employed.

Blue vitriol obtained equal to the cost of the acid.

If any parts of the alloy remained undissolved, it should be separated by decantation, and reserved for a future operation.

Undissolved alloy to be set by.

SCIENTIFIC

SCIENTIFIC NEWS.

Extract of a Letter from BRUGNATELLI, *concerning the non-existence of the charged Pile* *.

VOLTA has made many experiments on piles composed of a single metal, and a single wet stratum, which, from being inactive by themselves, become more or less active after affording a passage for a longer or shorter space of time to an electric current set in motion by active pile, &c.

Ritter asserted that a pile composed of one fluid, and one metal, was capable of being charged by another pile, but the fact is, that the fluid is converted into two different fluids by the electric current.

Ritter, the most judicious of the galvanic philosophers of Germany, has asserted, as Volta says, that the active pile, or common electrometer, transmits a real charge to the pile that is itself inactive, which it therefore calls the *charged pile*. Volta however has convinced himself, that no charge is transmitted, but, by virtue of the ordinary chemical action, the electric current being continued, changes the single wet stratum interposed between two pieces of gold, for example, into two different fluids, one acid, by which the electric current issues out of the metal, and the other alcaline, by which it enters; which constitutes a pile of the second order, namely, of one metal and two fluids of different natures, the action of which however does not continue long, because the fluids soon mix.

Mechanical work by Mr. Gregory.

Mr. GREGORY of the Royal Military Academy, Woolwich, has now in the press a Treatise on Mechanics, which is intended to be published in two volumes octavo. The first will be devoted chiefly to the theory, and will be divided into five books under the several heads of statics, dynamics, hydrostatics, hydrodynamics, and pneumatics. The second volume will be chiefly appropriated to practical and descriptive subjects, and will commence with general remarks, rules, and tables, relative to the nature, construction, and simplification of machinery; the effects of friction, and the rigidity of cords; and estimates of the varied energy of different first movers, &c. These will be followed by descriptions arranged alphabetically, of about 100 of the most curious, useful, and important machines. In this latter part, Mr. Gregory has been promised communications from some celebrated civil engineers, so that he hopes, on the whole, to render the work in some measure deserving the attention of those who are engaged in the cultivation and improvement either of the theory or the practice of mechanics.

* Van Mons *Journal de Chimie*, Vol. VI. p. 132.

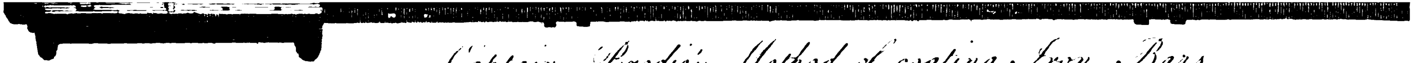

Fig. 1.

Captain Brodie's Method of coating Iron Bars.

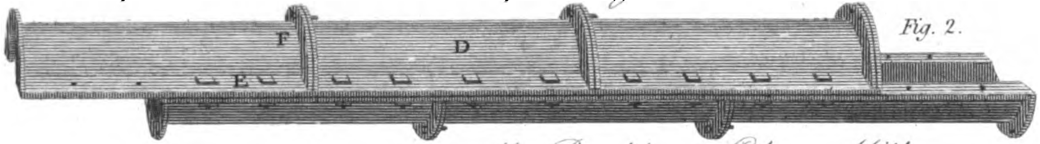

Fig. 2.

Mr. Rawlinson's Colour Mill.

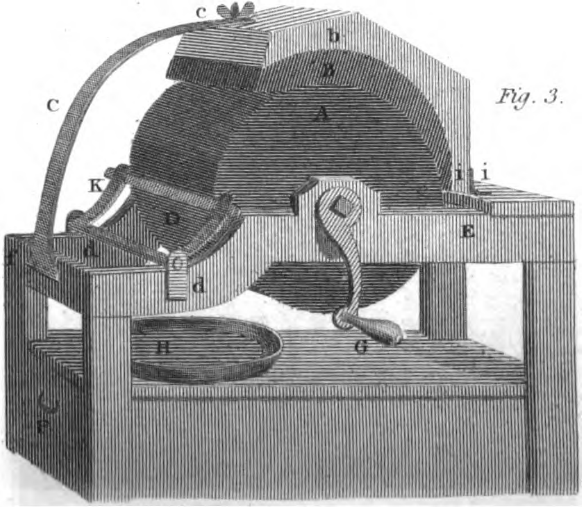

Fig. 3.

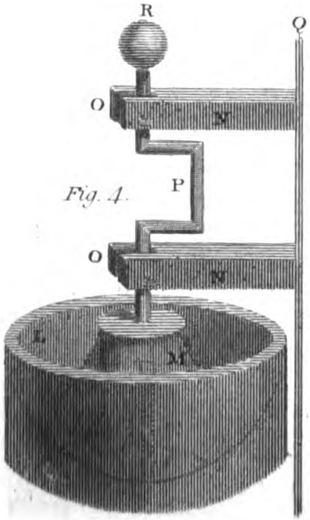

Fig. 4.

Fig. 5.

Improved Indian Mill

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JULY, 1805.

ARTICLE I.

Remarks on the Estimation of the Strength of Horses. In a Letter from Mr. O. GREGORY, *of the Royal Military Academy, Woolwich.*

To Mr. NICHOLSON.

SIR,

THE remarks of your ingenious correspondent, Mr. Hornblower, on the various estimates of the *Power of a Horse*, and the absurdity of adopting a quantity so fluctuating and so difficult to ascertain, as a common measure by which the powers and effects of steam engines and other machines are to be estimated and compared, have induced me to throw together a few observations on the same subjects; the theoretic part of which, though familiar to most men of science, seems not to be always known, or at least recollected, by some persons who are employed in the practice; and which are altogether much at your service for insertion in the Journal, if you think them likely to be of any utility.

Reference to Mr. Hornblower's paper on horse powers.

Dr. Desaguliers has given another estimate of the labour of a horse, beside that mentioned by Mr. Hornblower, and which indeed does not seem very consistent with it; for in vol. II. p. 251. of his Experimental Philosophy, he affirms that a horse in an

Estimate of horse power by Desaguliers. 200lb eight hours per day, at $2\frac{1}{2}$ miles per hour; or $3\frac{2}{3}$ feet per second.

an advantageous situation, is able to draw 200lbs. eight hours a day, walking at the rate of 2½ miles in an hour, or $3\frac{2}{3}$ feet in a second. This statement of the power of a horse, though it is not so great as that which is arbitrarily assumed by Messrs. Boulton and Watt, exceeds the determination of M. Sauveur, who estimates the mean effort of a horse at 175 French, or 189 avoirdupois lbs. with a velocity of rather more than three feet per second; and it probably exceeds Mr. Smeaton's statement of 550lbs. moved 40 feet in a minute; though, as will soon be seen, we are not furnished with proper data to institute a comparison between these various results. It is probable, however, as observed by the ingenious contributor of the article at page 216, vol. IX. of your Journal, that "the lowest of these "performances is more than equal to the average power of a "horse employed in husbandry for eight hours per day." So far as my own observations on this point extend, I am inclined to conclude that the average work of a stout London cart horse, for eight hours in a day, is little if any more than 130lbs. moved at the rate of three feet in a second, or $2\frac{1}{12}$ miles per hour. But this it would be ridiculous to assume positively as a universal unit of measure, in a case where the causes of variety are so numerous, and my opportunities of experiment comparatively few. The estimate just given, it should be observed, is not intended to express what a horse can draw upon a wheel carriage, where friction alone is to be overcome, after the load is once put into motion, and where a horse will often draw much more than 1000lbs. but the weight which a horse would raise out of a well, &c. the animal acting by a horizontal line of traction turned into the vertical direction by a simple pully or roller, whose friction is reduced as much as possible.*

—by Sauveur 189lb. avoird. three feet per second,

—by the author 130lb. three feet per second.

Misapplication of the general principle that gain in power is loss in time.

Before we can institute any comparison between the results of different experiments, it will always be necessary to enquire what machine was interposed between the weight moved and the animal, in each case, that we may thence deduce the real velocity with which the animal moved, from the velocity of the weight or load given by the observations. This is too fre-

* The late Mr. More, Sec. to the Society of Arts, found by the interposition of a graduated spring instrument between the horse and his work, that the re-action was between 70 and 80lb. when the velocity was three miles in an hour. I think the work was ploughing. See Philos. Journal, quarto, Vol. III. 136. N.

quently

quently omitted in consequence of an implicit reliance upon a maxim, which, though highly useful under proper restrictions, is far from universal in its application. In the case before us, if we admit the maxim now alluded to, namely, that *what is gained in power is lost in time*, with regard to the machine through whose intervention the velocity of the weight is rendered different from that of the horse, it would be unsafe to adopt it in the appreciation of the varied energy of the animal when moving with different velocities. The reason of this is obvious. The energy of the horse is obliged to be employed not only in overcoming the weight or resistance which opposes his progress, but in part in moving *himself*; for the particles which constitute his frame possess weight and inertia, and therefore cannot be put into motion without effort. Hence it follows that there is a certain velocity, which may be denoted by U, with which, when the animal moves, his whole power will be employed in producing his own motion solely, without being able to move any other body. If a body whose mass is M be attached to the horse, so that he cannot move without giving an equal velocity to the extraneous body, the same effort being employed both in moving the animal and the mass attached; the velocity V, with which they move must necessarily be less. And if M be farther increased while the weight and energy of the horse continue the same, the velocity V will be still farther diminished; and thus as M increases V will diminish, until when M arrives at a certain magnitude, W, the animal is unable to make any progressive motion, and exerts his force at what is called a dead pull. If M exceeds W, then will V become negative, and instead of the animal advancing with the load, the load will compel him to move backwards, and no useful work can be accomplished.

An horse, having himself to carry, does not exert his force against his work only.

Now these circumstances may be expressed algebraically, by the general formula $M \propto (U-V)^n$, in which the exponent n can only be determined by means of judicious and numerous experiments, where the magnitude of M should be ascertained for many variable values of V between the terms $V=U$, and $V=0$. From this theorem, following the common rules for the maxima and minima of quantities, it may readily be found that in order to have the *useful* work done the greatest possible, we must increase or decrease the weight till V becomes $=\frac{U}{n+1}$, when

Method of computing the effect of horses.

when the performance will be denoted by $\dfrac{n^n}{(n+1)^{n+1}} \times \mathrm{W\,U}$

And if the value of V thus exhibited be once ascertained experimentally, we need never be apprehensive of a material loss by a small variation from it; for by a well known property of those quantities which admit of a proper maximum and minimum, a value assumed at a moderate distance from either of these extremes will produce no sensible change in the effect.

Example.

In some of the actions of men, such as dragging a boat along a canal, &c. the value of n in the preceding theorems has been found to be nearly $= 2$. And the draught of horses is conformable to a law not widely different. The best experiments which have yet been made on this point with regard to horses drawing in nearly rectilinear paths, lead us to conclude that n is then very nearly $= \frac{9}{4}$, in the expression $\mathrm{M} \propto (\mathrm{U}-\mathrm{V})^{n}$. Assuming therefore for the utmost walking velocity of a horse, the value $\mathrm{U} = 9$ feet per second, a value which is quite high enough, any proposed estimates of the strength of this animal may be compared with facility. Thus, for example, let us enquire which is greater, the estimate of Mr. More (mentioned by Mr. Hornblower) of 80lbs. three miles per hour, or $4\frac{2}{5}$ feet per second; or that of 130lbs. moved at the rate of three feet per second? Here we shall have

$$(9-3)^{\frac{9}{4}} : (9-4{\cdot}4)^{\frac{9}{4}} :: 130 : 71\tfrac{1}{2}\text{lbs. nearly.}$$

The operation may easily be performed by means of a table of logarithms, and shews that the mean estimate I have laid down, when reduced to the same velocity as that by Mr. Moore, furnishes a result less than his by $8\frac{1}{2}$lbs. Which of these is the most accurate can only be determined by future experiments.

On the power of horses walking in circular paths;

If, however, either of these estimates should be adopted, it may be proper to remark that they would not hold with regard to the power of horses working in circular paths; yet, if it be at all proper to use *horse powers* in estimating the energy of machines, it seems most natural to take these powers as exerted by the animal in a round walk; so that it is still necessary to have a series of experiments to determine the values of n, and the relation of M and V when horses draw in circular walks of different radii. I say, of *different* radii, because it is certain that *cæteris paribus*, the greater the radius of the circle in

—of different radii.

in whose circumference the animal moves, the less fatigue will be occasioned by that kind of motion. Indeed it is obvious, that since a rectilinear motion is the most easy and natural for the horse, the less the line in which he moves is curved, with the greater facility he will walk over it, and the less he need recline from the vertical position. Besides this, with equal velocity at the circumference, the centrifugal force will be less in the greater circle, which will proportionally diminish the friction of the cylindrical part of the trunnions, and the labour of moving the machine. And farther, the greater the radius of the horse-walk, the nearer the chord of the circle in which the horse draws is to coincidence with the tangent, which is the most advantageous position of the line of traction. Hence it follows, that although a horse *may* draw in a walk of 18 feet diameter, yet he will work with far greater ease in one whose diameter is 35 or 40 feet; and it is very desireable that an experimental enquiry should be made to ascertain the proportion and absolute quantity of work in different circles. The larger walks are most advantageous.

I am of opinion that it would not be difficult to make some useful experiments, while work was actually carrying on at any horse mill, or machine where horses are constrained to move in a circular walk. The simple drawing which accompanies this letter will assist in conveying a clear idea of the method which I fancy might be advantageously adopted. Let AB *Fig.* 1. *Plate* X. be the vertical shaft to which the horizontal horse poles AC, AD, are attached. Let one horse work the machine by drawing at the ear E; but, instead of the transverse bar to which the harness is fixed being simply hung upon the hook *h*, let a good spring steelyard be interposed between that cross-bar and the hook, the graduations of which shall, when the machinery is put into motion, indicate the resistance (in lbs.) overcome by the animal, including the weight of the mass moved, the friction, &c. Near the extremity of the opposite horse-pole AD, let there be fixed a strong and correct common steelyard, whose divisions shall shew the various weights from 40 or 50 to 200lbs. and whose centre of motion shall be at the point *f* on the fixed stand. Let the cord *c* which is fastened to the shorter arm of this steelyard, pass (with as little friction as possible) over the pulley *p*, and thus, being turned into the horizontal direction, or rather, inclining a little upwards, let it be fixed to the cross bar Description of a mechanical apparatus for measuring the re-action in mills.

Description of a mechanical apparatus for measuring the re-action in mills.

bar of the harness of a second horse, equal in point of strength to the former. Then if the two horses, thus attached to the ears E and F, be made to pass over the walk in the same direction, following each other constantly at the distance of a semi-circumference; while that which draws at the ear E overcomes the whole pressure and resistance opposed by the work, the other which draws at F by the cord over the pulley *p*, will raise the weight *w* of the steelyard; which therefore, by being moved to and fro upon the arm *f i*, may be brought to exhibit an exact counterpoise, or measure of the exertion and power of the horse. And in order to ensure the greatest degree of accuracy in this respect, the motion of the two animals, and the position of the weight *w*, should be so adjusted, that the same weight should be shewn by the graduations both of the spring and of the lever steelyard. The shaking of the machinery will in some measure disturb the effect; but an ingenious manager of the experiments will find means of checking this: and as to the centrifugal force to which the weight *w* is exposed, it will never be of any material consequence in any of the slow motions which will be produced by this kind of work.

Each experiment should occupy the space of a fair day's work for the horses: for the conclusions deduced from shorter and irregular efforts are always erroneous in excess, and should be guarded against. The rate at which the animals move may readily be ascertained from the known circumference of the walk, and the number of rounds they are observed to make in 10 or 15 minutes. Thus, by continuing the experiments day after day, varying the velocity of the motion in some cases, and the radius of walk in others, such a series of results might at length be obtained, as would in a great measure remove the obscurity and doubt in which this business is at present enveloped. It is scarcely necessary to suggest the propriety of making a few experiments with a view of determining how far a load upon the back of each draught horse, would assist him in his labour. Nor can it be requisite to point out in what way, by means of such steelyards properly applied to waggons, &c. upon tolerably smooth roads, and two horses marching abreast, (one drawing the load, the other raising the weight,) experiments might be instituted to ascertain the magnitude of the efforts of horses when drawing

in

in rectilinear paths. Judicious experiments having these purposes in view, would certainly be beneficial, as they would enable us to tell what advantages might be expected from the labours of this useful quadruped in different circumstances. But with respect to the adoption of "horse power" as a unit of force in estimating the power of steam engines, &c. I confess that if it were as well known, and as unvariable as the length of the day of the equator, I should feel an aversion to applying it to any such purpose. It is a common measure arbitrarily adopted, which has no necessary connection with the subject that is referred to it, which does not in any respect facilitate the computation of the powers of an engine, and which may, without proper caution, lead to considerable errors in the conclusions deduced from it.

The adoption of an arbitrary unit from the power of an horse.

Before I close this letter, already perhaps too long, I beg permission to say a few words respecting the measure which is generally employed to determine the mechanical effect produced. This is the measure of the deservedly celebrated Mr. John Smeaton, who says that "the weight of a body multiplied by the height through which it descends, while driving a machine, is the only proper measure of the power expended; and that the weight multiplied by the height through which it is uniformly raised is the only proper measure of the effect produced." Mr. Smeaton was led to the use of this measure by his professional habits; and many who in this respect pay too great a deference to his authority, have adopted this measure as universal and preferable to any other. Taking this as a popular measure easy to recollect, and simple in its application, it undoubtedly has its uses; but in many instances it is inadequate to the purpose for which it is proposed. The late Professor Robison has some excellent observations on this subject, in the article *Machinery*, *Sup. Encyclopædia Britan.* where he lays down the just measure to which the scientific investigator will generally have recourse. "We take, says he, for the measure, (as it is the effect) of exerted mechanical power, the quantity of motion which it produces (or whose accumulation it prevents) by its uniform exertion during some given time. We say *uniform exertion*, not because this uniformity is necessary, but only because, if any variation of the exertion has taken place, it must be known in order to judge of the power."

Smeaton's measure of mechanical power and effect. He says it is as the product of the weight into the height passed through in either case;

rectified by Professor Robison.

A single

Smeaton's measure is not applicable to an horse sustaining (but not raising) a weight,

A single instance may be adduced, to which the measure of Mr. Smeaton is inapplicable, and in which we must have recourse to some such measure as that mentioned by Professor Robison. Suppose that a horse while standing still sustains by means of a rope and simple fixed pulley, a mass of a hundred-weight, and thus keeps it suspended at the top of a well, for the space of a minute. Neither the animal nor the weight moves, but shall we therefore say, in conformity, as it would seem, with Mr. Smeaton's measure, that there is no power expended, and no effect produced? On the contrary we know there is a power expended, and that the effort if sufficiently long continued would completely tire the horse. The effect which is produced is the annihilation of the simultaneous action of gravity upon the suspended mass; consequently, the effect produced is equal, and contrary to the momentum that would be generated by gravity in the space of a minute. So that $60 \times 32\frac{1}{6} \times 112 = 216160$, is the proper representative of the power expended, as well as of the work done. Were the rope to be cut and the weight suffered to fall for a minute, the same number would likewise denote the labour of the horse in restoring it to its original place, provided that could be accomplished in an equal space of time, without the horse changing his situation.

General statement.

It may not, perhaps, be entirely useless to state this matter rather more universally. To this end, let M represent any mass or body, $g = 32\frac{1}{6}$ feet, the velocity communicated to a body falling freely in the first second of time, and t' an indefinitely small portion of any time whatever t. Then will $g\,t'$ be the velocity generated in the instant t', and $M\,g\,t'$ the corresponding quantity of motion; this, therefore, measures the effort which must be exerted at each instant to sustain the weight, whether that effort be applied immediately, or through the intervention of a single fixed pulley. Hence it follows, that during the whole time t, the force will have consumed a quantity of motion equal to $\int M\,g\,t' = M\,g\,t$: that is to say, if t denote the time at the end of which the agent is no longer able to sustain the mass M, we may regard $M\,g\,t$ as being an adequate measure of the force ϕ of that agent. If the agent not only prevent the mass from falling, but actually raise it with a given uniform velocity V during the whole time t, then we must add the quantity of motion MV to the former, which

which gives $\phi = MV \times Mgt \pm M(V \times gt.)$ And lastly, if the agent possess inertia, its mass must also be considered. Thus, in the case of a horse whose mass is H, moving along with the velocity V during the time t, and raising the mass M, we shall have $\phi = (M \times H) V \times Mgt$. And from similar principles formulæ may be investigated to represent the power of a first mover in more complicated cases.

It will after all, be proper to distinguish carefully between the quantity of power expended, and that portion of it which is *usefully* employed: but a due consideration of this would too widely extend the limits of the present communication. Indeed I ought to apologize to yourself, and the scientific part of your readers, for dwelling so long as I have done upon topics which are well known to all who are conversant in the theory of mechanics: but if those, for whose use this letter is chiefly intended, shall derive some precise information, or add to the stock of their practical knowledge, by any hints of mine, I shall not fear being heavily censured for having entered thus into minutiæ.

Power expended and power usefully employed require to be distinguished.

I am, Sir,

Your's very respectfully,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich.
June 10*th*, 1805.

II.

An Account of some Analytical Experiments on a Mineral Production from Devonshire consisting principally of Alumine and Water. By HUMPHRY DAVY, *Esq. F. R. S. Professor of Chemistry in the Royal Institution. From the Philosophical Transactions in* 1805.

I. *Preliminary Observations.*

THIS fossil was found many years ago by Dr. Wavell, in a quarry near Barnstaple: Mr. Hatchett, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous schist. When first made known,

History of a Fossil.

it

it was considered as a zeolite; Mr. Hatchett, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. Babington, from its physical characters, and from some experiments on its solutions in acids, made at his request by Mr. Stocker, ascertained that it was a mineral body, as yet not described, and that it contained a considerable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

II. *Sensible Characters of the Fossil.*

Its sensible characters. Radiated hemispherical groups; white, silky, hard, little tenacious.

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common center, and inserted on the surface of the shift; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (apparently beginning to decompose) of yellow. Its lustre is silky; some of the specimens possess semi-transparency, but in general it is nearly opaque. Its texture is loose, but its small fragments possess great hardness, so as to scratch agate.

Other characters.

It produces no effect on the smell when breathed upon, has no taste, does not become electrical or phosphorescent by heat or friction, and does not adhere to the tongue till after it has been strongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loses its hardness, and becomes opaque. In consequence of the minuteness of the portions which it is found, few of them exceeding the size of a pea, it is very difficult to ascertain its specific gravity with any precision; but from several trials I am disposed to believe, that it does not exceed 2,70, that of water being considered as 1,00.

III. *Chemical Characters of the Fossil.*

Chemical habitudes. Soluble in acids and in f. alkalis.

The perfectly white and semi-transparent specimens of the fossil are soluble both in the mineral acids and in fixed alkaline lixivia by heat, without sensibly effervescing and without leaving

leaving any notable refiduum; but a fmall part remains undiffolved, when coloured or opaque fpecimens are expofed to the alkaline lixivia.

A fmall femi-tranfparent piece, acted on by the higheft heat of an excellent forge, had its cryftalline texture deftroyed, and was rendered opaque; but it did not enter into fufion. After the experiment it adhered ftrongly to the tongue, and was found to have loft more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the foffil. When it was acted on by a heat of from 212° to 600° Fahrenheit in a glafs tube, it gave out an elaftic vapour, which when condenfed appeared as a clear fluid poffeffing a flight empyreumatic fmell, but no tafte different from that of pure water.

Forge heat gave opacity, but did not fufe it. Lofs one-fourth.

It emitted water.

The folution of the foffil in fulphuric acid, when evaporated fufficiently, depofited cryftals which appeared in thin plates, and had all the properties of fulphate of alumine; and the folid matter, when rediffolved and mixed with a little carbonate of potafh, flowly depofited octahedral cryftals of alum. The folid matter precipitated from the folution of the white and femi-tranfparent foffil in muriatic acid, was in no manner acted upon by folution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect folubility without refiduum in alkaline lixivia fhewed that it was alumine.

Sulphuric folution afforded fulphate of alumine.

Muriatic folution contained alumine *only*.

When the opaque *varieties* of the foffil were fully expofed to the agency of alkaline lixivia, the refiduum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when diffolved in muriatic acid, not in excefs, it gave a white precipitate when mixed with folution of oxalate of ammonia, and did not affect folution of pruffiate of potafh and iron.

The white varieties contained fome lime;

In the green opaque variety, calcareous earth was indicated by folution of oxalate of ammonia: and it contained oxide of manganefe; for it was not precipitated by folution of ammonia; but was rendered turbid, and of a gray colour, by folution of pruffiate of potafh and iron.

and the green, alfo manganefe:

The refiduum of the alkaline folution of the yellow variety, when diffolved in muriatic acid, produced a fmall quantity of white folid matter when mixed with the folution of the oxalate

the yellow, a trace of iron.

of

of ammonia, and gave a light yellow precipitate by exposure to ammonia; but after this, when neutralized, it did not affect prussiate of potash and iron, so that its colouring matter, as there is every reason to believe, was *oxide of iron.*

IV. *Analysis of the Fossil.*

Analysis of 80 grains. 1. Exposure to heat.

Eighty grains of the fossil consisting of the whitest and most transparent parts that could be obtained, were introduced into a small glass tube having a bulb of sufficient capacity to receive them with great ease. To the end of this tube, a small glass globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fusion by means of the blow-pipe.

The bulb of the tube was exposed to the heat of an Argand lamp; and the globe was preserved cool by being placed in a vessel of cold water. In consequence of this arrangement, the fluid disengaged by the heat, became condensed, and no elastic matter could be lost. The process was continued for half an hour, when the glass tube was quite red.

Water came over. 19 grains

A very minute portion only of permanently elastic fluid passed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another vessel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried the whole loss indicated was 21 grains. The 19 grains of fluid had a faint smell, similar to that of burning peat; it was transparent, and tasted like distilled water: but it slightly reddened litmus paper. It produced no cloudiness in solutions of muriate of barytes, of acetite lead, of nitrate of silver, or of sulphate of iron.

very slightly acid.

2. Solution of the residue in sulph. acid: precipitation and re-solution by alkali. One grain and a quarter of lime remained undissolved.

The 59 grains of solid matter were dissolved in diluted sulphuric acid, which left no residuum; and the solution was mixed with potash, in sufficient quantity to cause the alumine at first precipitated again to dissolve. What remained undissolved by potash, after being collected and properly washed, was heated strongly and weighed; its quantity was a grain and quarter. It was white, caustic to the taste, and had all the properties of lime.

3. Nitric acid was added in excess, and then carbonate of am-

The solution was mixed with nitric acid till it *became sour.* Solution of carbonate of ammonia was then poured into it till the effect of decomposition ceased. The whole thrown into a filtrating

filtrating apparatus left solid matter, which when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general results of the experiments, when calculated upon, indicated for 100 parts of this specimen,

monia. The precip. of alumine, after ignition, was 56 grains.

Of alumine	-	-	70
Of lime	-	-	1.4
Of fluid	-	-	26.2
Loss	-	-	2.4

Component parts.

The loss I am inclined to attribute to some fluid remaining in the stone after the process of distillation; for I have found, from several experiments, that a red heat is not sufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a strong white heat.

The loss appears to have been volatilizable matter.

Fifty grains of a very transparent part of the fossil, by being exposed in a red heat for fifteen minutes, lost 13 grains; but when they were heated to whiteness, the deficiency amounted to 15 grains, and the case was similar in other trials.

Different specimens of the fossil were examined with great care, for the purpose of ascertaining whether any minute portion of fixed alkali existed in them; but no indications of this substance could be observed; the processes were conducted by means of solution of the unaltered fossil in nitric acid; the earths and oxides were precipitated from the solution by being boiled with carbonate of ammonia; and after their separation, the fluid was evaporated to dryness, and the nitrate of ammonia decomposed by heat, when no residuum occurred.

The mineral contains no alkali.

A comparative analysis of 30 grains of a very pellucid specimen was made by solution in lixivium of potash. This specimen lost 8 grains by long-continued ignition, after which it easily dissolved in the lixivium by heat, leaving a residuum of a quarter of a grain only, which was red oxide of iron. The precipitate from the solution of potash, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Other analyses.

Several specimens were distilled in the manner above described, and in all cases the water collected had similar properties. The only test by which the presence of acid matter in it could be detected, was litmus paper; and in some cases the effect upon this substance was barely perceptible.

V. *General*

V. *General Observations.*

The acid matter in the water was not nitric, mur. or sulph.

I have made several experiments with the hope of ascertaining the nature of the acid matter in the water; but from the impossibility of procuring any considerable quantity of the fossil, they have been wholly unsuccessful. It is, however evident, from the experiments already detailed, that it is not one of the known mineral acids.

It is foreign to the stone.

I am disposed to believe, from the minuteness of its proportion, and from the difference of this proportion in different cases, that it is not essential to the composition of the stone and that, as well as the oxide of manganese, that of iron and the lime it is only an accidental ingredient, and on this idea the pure matter of the fossil must be considered as a chemical combination *of about thirty parts of water and seventy of alumine.*

Alumine has an affinity for water.

The experiments of M. Theodore de Saussure on the precipitation * of alumine from its solutions, have demonstrated the affinity of this body for water; but as yet I believe no aluminous stone, except that which I have just described, has been found, containing so large a proportion of water, as thirty parts in the hundred.

Diaspore exam. by Vauquelin, contains 80 alumine and 16 water.

The diaspore, which has been examined by M. Vauquelin, and which loses sixteen or seventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is supposed by that excellent chemist to be a compound of alumine and water. Its physical and chemical characters differ however very much from those of the new fossil, and other researches are wanting to ascertain whether the part of it volatilized by heat is of the same kind.

Cornish mineral resembling the subject of this paper.

I have examined a fossil from near St. Austle, in Cornwall, very similar to the fossil from Barnstaple in all *its general chemical characters;* and I have been informed, that an analysis of it, made by the Rev. William Gregor some months since, proves that it consists of similar ingredients.

Proposed names.

Dr. Babington has proposed to call the fossil from Devonshire *Wavellite,* from Dr. Wavell, the gentleman who discovered it; but if a name founded upon its chemical composition be preferred, it may be denominated *Hydrargillite,* from ὕδωρ water, and ἄργιλλος clay.

* Journal de Physique, Tom. LII. p. 280.

On

III.

On the Aberrations of Light passing through Lenses. By Mr. EZEKIEL WALKER.

THE discovery of the aberration of the rays of light, caused by their unequal refrangibility, formed a new area in the science of optics. It is the foundation of all *Sir Isaac Newton's* discoveries in light and colours; and also the foundation of most of the useful improvements in the construction of optical instruments, that have done so much honour to our country. And this science may still derive further improvements from the same discovery, not only in the construction of instruments, but also in explaining some curious phenomena in nature. Newton's discovery of the aberrations of coloured rays.

But before I attempt to show how the use of this property of vision may be extended, it seems necessary to give a short account of that kind of aberration which arises from the unequal refrangibility of the differently coloured rays of light: the other aberration, or that which is caused by the spherical figure of the lens, is not here considered as being inconsiderable when compared with the former.

Therefore let A C B, *Fig.* 2, *Plate* X. represent a plano-convex lens; P A and R B two pencils of white or compound rays of light, falling upon it at the points A and B in a direction parallel to its axis. Also let A xv and B yv be the red or least refrangible rays, and A gy and B qx the violet, or most refrangible. The red ray from A will cut the violet ray from B at the point x, and the red ray from B will cut the violet ray from A at the point y; through these intersections draw the line xy, and this line will be the diameter of the least circular space into which all the rays that fall upon the lens, parallel to its axis, can be collected. And this circle, which for brevity's sake is called the circle of aberration, is the true focus of the lens or place where the image of the object is formed. Aberration of light that passes through a lens.

Let the sine of incidence going out of glass be n, the sines of refraction (into air) of the least and the most refrangible rays be p and q; then if a plano-convex lens be exposed with the plane side to the sun, the diameter of the circle of aberration xy (or image of the sun formed of rays of different refrangibility) is to the diameter of the lens A B, as $q-p$ to $q\times p-2n$.*

* This theorem is well known to mathematicians.

From

From this given ratio of A B to xy it follows, that the image of the sun within a telescope varies with the aperture of the object glass.

Sir Isaac Newton found by most accurate experiments that where the sine of incidence was 50, the sines of refraction of the red and the violet rays were 77 and 78. Hence $q-p$ is to $q+p-2n$ as 1 to 55. And therefore A B : xy : : 55 : 1; or $xy = \frac{AB}{55}$.

To elucidate this theorem by examples, let the diameter of the object glass of a telescope, which is 4 inches, be contracted to 3 inches, and afterwards to 2; then the diameters of the circles by aberration formed by parallel rays, will be $\frac{4}{55} = .072$, $\frac{3}{55} = .054$, and $\frac{2}{55} = .036$ respectively.

The same Property of Vision demonstrated otherwise.

Thus, let n represent the sine of incidence, and p and q the sines of refraction, as before.

The sine of incidence of every ray, is to its sine of refraction in a given ratio.*

And the sine of incidence of the extreme ray P A, varies with the aperture of the lens. For n becomes less as P A approaches the axis of the lens E v.

Therefore the sines of refraction p and q, the angle x A y, and its subtense xy increase or decrease with A B. Consequently the image of the sun or moon, upon the retina increases or decreases in magnitude with the pupil of the eye.

Deduction; Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of A B to xy, in which they increase or decrease at the same time that the image of a candle formed in the focus of a convex lens decreases with the aperture of the glass. For the rays of the sun and the light of a candle are both governed by the same law, in the formation of images in the focus of a lens; but this law does not obtain in the same degree in both objects, in consequence of the rays of the latter being in a more diverging state than those of the former.

—applied in support of the author's experiments. Hence the truth of the result of my experiments, which were published in Vol. IX. page 164 of this Journal, is proved from the discoveries of *Sir Isaac Newton.*

* Newton's Optics, page 64.

4 What

What I have further to advance on this subject must be reserved until some future opportunity, as it would exceed the limits of this paper.

E. WALKER.

Lynn, May 16, 1805.

IV.

New Method of decomposing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. GOETTLING.*

Muriate of barytes much used.

THE muriate of barytes is now in such general use, that every improvement in the mode of preparing it must meet a favourable reception. This will render the new method of Mr. Goettling acceptable to the public.

Decomposition of sulphate of barytes by charcoal troublesome and incomplete.

The decomposition of sulphate of barytes by means of charcoal requires a strong fire continued a long time, and never succeeds completely. This is owing on the one hand to the strongly oxigenated quality of the acidifying principle in the sulphuric acid, so that in its translation to the charcoal it gives out but little caloric; and on the other hand to the difficulty of imparting a certain degree of heat to a mixture, into which a large quantity of a body that is so bad a conductor of heat as charcoal enters. To remedy the first of these defects, I had already proposed to increase the proportion of charcoal a little, and to incorporate with the mixture of charcoal and sulphate of barytes a twentieth of nitrate of potash. To remedy the second, Mr. Goettling advises to add muriate of soda to the mixture, which serves at the same time as a conductor of heat and a flux. The following is his method.

Remedies: to use more charcoal and nitrate of potash; —and muriate of soda.

Mr. Goettling's process. Sulphate of barytes with muriate of soda and some charcoal are heated together.

Four parts of native sulphate of barytes in fine powder are to be mixed with one part of muriate of soda and half a part of charcoal powder. This mixture is to be pressed hard into a Hessian crucible, and exposed for an hour and half to a red heat in a good wind furnace. After it has grown cold, the

* Translated from Van Mons's *Journal de Chimie*, Vol. VI. p. 80. Originally published in the *Taschen-Buch fuer Scheidekuenstler*.

mass

The saline mass is then to be dissolved.

mass is to be reduced to a coarse powder, and boiled for a moment with fixteen parts of water. The liquor is then to be filtered, and kept in well stopped bottles.

The time of exposure to heat may be shortened to one half, if the quantity of muriate of soda be doubled, and the matter occasionally stirred. In this case too, double the quantity of water should be used to lixiviate the mass.

An addition of muriatic acid expels the sulphureous acid and leaves muriates of barytes and of soda.

To prepare muriate of barytes with this lixivium of sulphuret of barytes, which at the same time holds in solution muriate of soda, muriatic acid is to be added in separate portions, till sulphurated hidrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the residuum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afresh, is to be set to crystallize; the muriate of soda, which is much more soluble in water than the muriate of barytes, and not more soluble with heat than without, is not deposited by cooling, and the muriate of barytes crystallizes alone.

These crystals of muriate of barytes are separated by cooling after evaporation.

The remaining lixivium is to be evaporated and set to crystallize again, and this is to be repeated till no more crystals of muriate of barytes are formed.

The barytic salt is perfectly white and pure.

The barytic salt thus obtained, if care be taken not to employ an excess of muriatic acid, is perfectly white, on account of the hydrosulphuret, by which the iron and other metallic substances are precipitated. To be more certain that it contains no muriate of soda, the different products of the crystallization should be mixed together, dissolved, and re-crystallized.

V.

On the Action of Platina and Mercury upon each other. By RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A, &c. From the Philosophical Transactions for* 1805, *p.* 104.

Freyberg, June 3d, 1804.

Reference to the author's paper on palladium.

ON the 12th of May, 1803, I had the honour of presenting a Paper to the Royal Society,* the object of which was to discover the nature of palladium, a substance just then announced

* Inserted in our Journal, Vol. VII. 85, 176.

to

to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

Attempts to repeat his experiments have been unsuccessful.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of enquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

Dr. Wollaston denies their accuracy, but has not yet published his own.

The first doubts were manifested in England; and Dr. Wollaston very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium, if I had not mentioned it in this place.

Guyton, in France, coincides with the author.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be made upon them, and M. Guyton was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Vauquelin and Fourcroy,

Messrs. Vauquelin and Fourcroy then undertook the subject, and they were led by it to the confirmation of the recent discovery of Monf. Descotils. The existence of a new metal, which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowlege as simple.

—infer that palladium contains no mercury; but is platina with the new metal of Descotils.

No fooner were thefe celebrated chemifts convinced of the exiftence of a new metal in platina, than they concluded that it muft play a principal part in the compofition of palladium. Shortly after this, in a note to a letter from M. Prouft to M. Vauquelin, in which M. Prouft expreffes his aftonifhment concerning all he has read upon palladium, Meff. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new fubftance does or does not play a principal part in the formation of palladium, could not be afcertained at the time my experiments were made, becaufe the new metal itfelf was not then known. But from all that Meff. Fourcroy and Vauquelin have ftated, in fuch of their different memoirs upon this fubject as I have feen, the grounds of their fuppofition have not appeared. May we not refer their opinion, then, to that common propenfity of the mind, againft which M. Fourcroy has himfelf warned us with equal juftnefs and eloquence on another occafion, namely, a pronenefs to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of univerfal influence.

Meff. Rofe and Gehlen,

Meff. Rofe and Gehlen* were the firft among the German chemifts who inftituted experiments upon palladium; and M. Richter has alfo publifhed a paper on the fame fubject.

—attempted without fuccefs to form palladium by precipitating a mixed folution of platina and mercury by gr. fulphate of iron;

The firft attempt of Meff. Rofe and Gehlen to form palladium was by the precipitation of a mixed folution of platina and mercury by green fulphate of iron. Their refult was precifely that which I had obferved when my operations failed altogether, and which of courfe was the moft frequent. This method was repeated twice. The fecond time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more fuccefsful than the former.

—and alfo by paffing fulphuretted hidrogen through the mixed folution;

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by paffing a current of fulphuretted hydrogen gas through a mixed folution of platina and mercury. Their method was the following.

* Neues Algemeines Journal der Chemie herausgegeben von Hermftadt, Klaproth, Richter, Scherer, Tromfdorff, und Gehlen. Erften bandes funftes heft.

They

They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was re-dissolved. The liquor was then filtered, and to that part of it, which contained the re-dissolved precipitate, an acid was added. From this process they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this exposed to a strong heat, left three-eighths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been re-dissolved; and the result of the experiment was complete failure.

by adding hydrosulph. of potash to the m. solution;

—but they obtained no palladium.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat, they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals, as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former paper.

They did not exactly repeat the author's experiment. He used the pure gas; they added an alkali;

The sulphuretted hydrogen gas which Messrs, Rose and Gehlen presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered by M. Vauquelin at the Ecole des Mines in Paris, when he was Professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydrosulphurets, was redissolved by adding an excess of them.

—and this last substance acting not only on mercury;

It

—but on the platina;

It is moreover well known, that there is a ſtrong affinity between potaſh and the oxide of platina, and alſo that when thoſe ſubſtances are brought together in ſolution, a triple ſalt, but little ſoluble, is the reſult. It was to avoid theſe difficulties that I had employed uncombined ſulphuretted hidrogen gas; for the method adopted by Meſſ. Roſe and Gehlen appearing to me to be the application of two divellent forces, I preſumed that it would produce a ſeparation. The reſult of their experiment, which, it appears from their paper, they had not anticipated, ſhews the neceſſity of the precaution I had uſed. The operation which they performed to unite platina and mercury was, in fact, nearly the reverſe of that which they ſuppoſed they had repeated from me, and might have been applied perhaps with a better proſpect of ſucceſs towards the decompoſition of palladium.

—would have an effect nearly oppoſite to that of the author's proceſs.

They could not fuſe platina.

Meſſ. Roſe and Gehlen ſeem, in many parts of their paper, to queſtion my having fuſed platina; and inform us that although they had expoſed this metal in the furnace of the Royal Porcelain Manufactory of Berlin, in which Wedgewood's pyrometer ceaſed to mark the degree of heat, they could not accompliſh its fuſion. Many of my friends in England have however ſeen the buttons which I obtained, and which were not few in number. The flux which I had uſed was borax. But no mention is made in any one of the operations of Meſſ. Roſe and Gehlen of borax having been employed.

The author uſed borax.

Particular account of his proceſs.

In many of their attempts they obtained an irregular and porous maſs, which of courſe was of a ſpecific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of ſpecific gravity, which I had obſerved, was owing to the ſame cauſe. It is true, not only that I had very often obtained ſuch a maſs, but that I had frequently alſo obſerved no diminution whatſoever in the ſpecific gravity of the button which reſulted from my operations. But all thoſe upon which I had founded the concluſions alluded to by Meſſ. Roſe and Gehlen were performed in the following manner, and have been repeated ſince. A Heſſian crucible was filled with lamp-black, and the contents preſſed hard together. The lamp-black was then hollowed out to the ſhape of the crucible as far as one-third from the bottom, leaving that much filled with the compreſſed materials; this lining, which adhered ſtrongly to the ſides of the crucible, was made extremely thin,

A Heſſian crucible was lined with lamp black;

in

in order not to obstruct the passage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick mass of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fused, or varied according to other circumstances at pleasure. In general the axis of the cylindrical hole was about three or four times the diameter of the basis. After withdrawing the rod the crucible was about half filled with borax. Upon this was placed the metal to be fused; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of course perpendicular to the axis of the cylindrical excavation at the bottom of the crucible. More borax was then added to cover the piece of metal, and another quantity of lamp-black was pressed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this state it was exposed to heat in a forge, in which upon another occasion, I had, in the presence of Mess. Hatchett, Howard, Davy, and others, completely melted a Hessian crucible lined and prepared in the same manner. The fuel which I used was the patent coak of Mess. Davey and Sawyer. In the present experiments I moderated the heat so as not materially to injure the crucible, and upon taking it out of the fire, the lining was generally found so compact and so firm that it remained in a solid mass after the crucible was broken. When the metallic cylinder occupied the space at the bottom, it was natural to suppose that it had been fused; because in no other state but that of liquidity could it have run into the mould. In order however to prevent all objections I had the precaution to make the hole of a different diameter from the metallic cylinder, and to observe whether the necessary change in the shape of the latter ensued. If after such a test, repeated as often as required, I perceived that the metal did not vary in its specific gravity, I thought myself authorised to conclude that it was exempt from air.

—a small cylindrical cavity was made in the lower part of the lining;

—the metal occupied the middle of a mass of borax that filled up the cavity of the vessel;

—strong heat was applied;

—and when the metal lost its first dimensions and occupied the cylindrical cavity it must have been fused.

M. Richter says that he had hoped to have put himself in possession of a considerable piece of palladium by repeating with minute accuracy the process which I had recommended as the best. He precipitated a mixed solution of platina and mercury by a solution of green sulphate of iron; and after varying the subsequent operations, to which he submitted the product

M. Richter's attempt and conclusions;

product he had obtained by this method, he was led to the following important conclusions amongst others of less consequence. 1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly. That mercury is capable of entering into combination with platina so, that it cannot afterwards be separated by fire. From the first of these conclusions it is evident, that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

Attempts of Tromsdorff and Klaproth: not successful.

In addition to the chemists abovementioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attemps to form this combination; and Klaproth, in a letter to M. Vauquelin published in the *Annales de Chemie*, for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Remarks. The chemists who expected to succeed in a few trials, could not have attended to the author's paper;

Mess. Rose and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the success of their experiments, which no words of mine had authorised, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance. M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my Paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure, and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success.

He made many hundreds of experiments, and succeeded completely but four times.

The course of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently seven

seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to have been one thousand. Amongst these I had four successful operations. I persevered, because even in my failures I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Mess. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been performed very often, these authors would not have neglected to mention it. M. Richter states his merely as preparatory to more extensive researches; and M. Tromsdorff, as well as M. Klaproth, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. Ritter, the celebrated Galvanist of Jena. M. Ritter had ascertained the rank which a greater number of substances hold in a galvanic series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur, pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led Mr. Ritter into a new and general train of reasoning, and induced him to undertake the examination of a great

M. Ritter the great galvanist, has established the order of galvanic relation in bodies.

He found palladium to be removed in that order from among the metals to the compound bodies.

great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher; and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose, that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. Ritter has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

Other interesting facts.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail: and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it: yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

The objectors to the author's conclusions have not paid attention to the repeated failures of all his methods.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago Professor Lampadius, in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitless attempts abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest excited by an experiment so likely to fail. Some time after this Mess. Clement and Desormes

A recent case in chemistry which is perfectly analogous to this.

Deformes obtained the fame refult, and attempted to produce the fubftance a fecond time. They performed a vaft number of experiments; but their fuccefs bore no proportion to their diligence and zeal. They publifhed an account of their procefs and its confequences, but gained little credit, as no perfon was fortunate enough to produce the fame fubftance. Many difbelieved the experiments altogether, and denied the exiftence of fuch a combination; whilft others, lefs inclined to doubt, attributed its formation to fortuitous circumftances which might never again occur together. In February, 1804, Profeffor Lampadius, in diftilling fome pyritized wood, though with a different intent, obtained the fame fubftance. As he had it now in his power to obferve the phenomena that attended its formation, he difcovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the fubject, as the neceffary precautions can be followed by every chemift, Mefs. Clement and Deformes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Profeffor Lampadius terms his fulphur-alcohol is no longer a refult of chance, or accounted for by being fuppofed one of thofe fubterfuges to which human pride reforts, in order to fpare itfelf the confeffion of human weaknefs.

Difcovery of the fulphur-alcohol of Lampadius, which he could not repeat: It was afterwards once only verified by Clement and Deformes.

The inventor re-difcovered the fubftance, and it can be produced at pleafure.

The obfervation of any new fact becomes a matter of general concern, and truly worthy of philofophic contemplation, then only, when its influence is likely to be extended beyond the fingle inftance to which it owes its difcovery. Whether water were a fimple body or a compound could have been of little importance as an infulated fact; but, connected with the vaft chain of reafoning it gave rife to, it opened a new field for genius to explore. If in the prefent cafe our refearches were to be confined merely to afcertaining whether palladium were a fimple metal or a compound, all the advantages likely to arife from the facts obferved during the inquiry would be loft; and an object of the moft comprehenfive intereft would thus fink into a controverfy concerning the exiftence of one more of thofe fubftances, which we have dignified with the name of elements. It was in this point of view that Mefs. Richter and Ritter confidered the fubject as far as they went, and a few

New facts are of value the more extenfive their relations, &c.

a few facts are stated in my first Paper in support of the opinion, that palladium is but a particular instance of a general truth.

Conclusion: that compound metals may be taken for simple, and the detection be extremely difficult.

By taking the reasoning on this subject then, in its widest extent, we shall be led, I think, to the following conclusion: That metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple; but to prove this must be a work of great time and perseverance; and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis therefore does not promise much success; and the labour of synthesis is sufficient to deter any individual from the undertaking.

Platina and mercury do act on each other, and disguise the properties of each.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

Example: Platina is precipitated in the metallic state by green sulph. of iron, if mercury or silver be present; but not else.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which

which does not happen when it is alone. I have tried to produce the fame effect with other metals and platina, but I have not obferved any thing fimilar. It is therefore fair to conclude, that when a folution of platina is precipitated in a metallic ftate by a folution of green fulphate of iron, either filver or mercury is prefent.

The precipitation of a mixed folution of platina and filver requires no further caution than to free the falt of platina as much as poffible from muriatic acid; for as I obferved in my former Paper, the effect of nitrate of filver poured into muriate of platina, is to produce a precipitate, not of muriate of filver, but of a triple muriate of platina and filver. It was by this experiment that I then proved the affinity of thefe two metals; for when filver is not prefent, muriate of platina is among the moft foluble falts. The beft method of prefenting the three folutions of platina, filver, and green fulphate of iron to each other, is firft to pour the filtered folution of the laft into the folution of platina, and then, after mixing them thoroughly together, to add the folution of filver by degrees, and to ftir them conftantly. In this, as in all fimilar operations, the prefence of all acids, falts, &c. excepting thofe neceffary for the operation, fhould be avoided; and if proper proportions have been ufed, and all circumftances attended to, the precipitation of thefe two metals will be very complete.

Nitrate of filver added to mur. platina throws down a triple muriate of pl. and filver.

Precife method of precip. pl. and filv. in the metallic ftate by fulph. iron.

But the precipitation by a folution of mercury requires to be further confidered, as the ftate of oxidizement of this metal, as well as the acid in which it is diffolved, produces a confiderable modification in the refult, In the firft place the oxide, at the minimum of oxidizement, diffolved in muriatic acid, is unfit for the experiment; and even the red oxide diffolved in the fame acid, or corrofive fublimate, is not the moft advantageous. When a warm folution of the latter is poured into a mixed folution of platina and green fulphate of iron alfo warm, as in the cafe of filver, thefe fubftances are brought into contact under the moft favourable circumftances. Yet even thus the precipitation is flowly and imperfectly formed, often not till feveral hours have elapfed; and fometimes a very great deficiency of weight is obferved, between the quantities ufed and thofe recovered directly by this method. If a folution of nitrate of mercury be ufed, the effect is produced more

The fame, but with folution of mercury.

more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former Paper.

Mercury dissolved at the minimum of oxidation will fall in a triple salt with platina without any addition of sulph. iron.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This triple salt of platina and mercury shall be presently examined. From this it is evident than to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green sulphate of iron is superfluous.

Not so if the mercury be at the maximum of oxidizement.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

Whether mercury be precipitable, singly, by green sulphate of iron? &c.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is not

not fensibly acted upon by the fame reagent. The infolubility of muriate of filver might be alledged as the caufe of this, if I had not tried the experiment by pouring nitrate of filver into green muriate of iron, in which cafe all the fubftances were prefented to each other in folution. The refult was not reduction, but muriate of filver and nitrate of iron. This fact refts upon a much more extenfive bafis than mere mechanical circumftances; and, if purfued with the intention it deferves, it would lead us into the wide expanfe of complicated affinities and their relations. From reafoning alone we fhould be difpofed to think that an acid, fo eafily decompofed as the nitric, would be fufficient to prevent the reduction of a metal which it can diffolve. But on the one hand it can fpend its oxygen upon a part of the oxide of the green fulphate of iron, while on the other its affinity for oxide of filver is not powerful enough to retain it, when there is another part of the oxide of iron prefent to deprive it of oxygen. But the affinity of muriatic acid for oxide of filver, one of the ftrongeft at prefent known, is fufficient to counterbalance all the other forces. There are many other inftances of the fame kind.

Mercury in nitric acid is precipitated (metallic) by gr. fulph. of iron.

If then a folution of green fulphate of iron be brought into contact with either foluble or infoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be diffolved in nitric acid, and green fulphate of iron be added, the mercury is precipitated in the metallic ftate.

Thefe remarkable facts modify the refults formerly ftated.

Thefe experiments are much ftronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general confideration but in their application to the prefent fubject. They moft materially modify and are indifpenfable to the accuracy of the refults I formerly ftated; but I was not aware of them at the time I firft engaged in the inveftigation of this fubject. I can alfo now explain a very material difference between fome proportions obferved by M. Richter and myfelf in an experiment which that chemift had made as a repetition of one of mine.

Precipitation of gold and mercury by gr. fulphate of iron: repeated, by Richter.

I had poured a folution of green fulphate of iron into a folution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate confifting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment;

ment; that is, he used 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his *method* of *repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury.

The state of oxidation of the nitrate of mercury used with the solution of gold is of consequence: If the minimum of oxidation prevails, calomel and metallic gold fall down;

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat.

but if the maximum, then nothing falls till the green sulphate of iron is added.

If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate of iron, cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

Experiments with platina.
1. Much of the highly oxid. sol. of mercury poured into a mixed solution of plat.

Exper. 1. If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in

in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat little more is left in general than the original platina; and sometimes even a diminution may be observed; for as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used. Before the precipitate has been exposed to heat it is dissolved more easily than platina by nitro-muriatic acid; and the solution when nearly in a neutral state gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a solution of green sulphate of iron.

and gr. sulph. of iron, throws down platina and mercury united.

Part of the latter is defended from nitric acid.

Heat drives off the mercury.

The compound precipitate is more soluble than plat. in n. m. acid, and gives much metal. precip. by add. of s. of gr. s. of iron.

Exper. 2. When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

Exp. 2. Metallic iron throws down the pl. and merc. from a mixed solution. Nitric acid does not deprive this metal of all its mercury. It is more soluble in n. m. acid; and precip. by gr. s. of iron. Heat usually expels the mercury from this.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count Mussin Puschkin to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in the

Exp. 3. Amalgam (from ammon. mur. of plat.) strongly heated, leaves a powder, acted on by n. m acid, and copiously

precip. by gr. f. of iron.

the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this paper.

Exp. 4. Sulphur added in the last Exp. causes a greater precip.

Exper. 4. If sulphur be added to the ingredients recommended by Count Mussin Pushkin, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the button which results from the operation, is generally more considerable.

Exp. 5. Sulphur rubbed with am. m. of plat. easily melts. Mercury being added, strong fusion gives a button, sol. in n. m. acid and precipitable by gr. f. of iron.

Exper. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exp. 6. The precip. from sol. of plat. and merc. by sulph. hydrogen being fused affords a sol. precip. by gr. f. of iron.

Exper. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exp. 7. So likewise the precip. by phosphate of ammon.

Exper. 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exp. 8. Nitr. of merc. at min. of oxidiz. precipitates mur. of pl. The metallic compound dissolved in n. m. acid is precipitable by gr. f. of iron.

Exper. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes place is independent of every substance except the metals themselves.

Exper.

Exper. 9. One of the most delicate tests that I have observed in chemistry is recent muriate of tin, which detects the presence of the smallest portion of mercury. When a single drop of a saturate solution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a saturate solution of recent muriate of tin are added, the liquor becomes a little turbid, and of a smoke-gray colour. If these 500 grains of liquid be diluted with ten times their weight of water, the effect is of course diminished, but still it is perceptible. I had on a former occasion observed the action of recent muriate of tin upon a solution of platina. If a solution of recent muriate of tin be poured into a mixed solution of platina and mercury, not too concentrated, it can hardly be distinguished from a simple solution of platina. But if too much mercury be present, the excess is acted upon as mercury; and the liquor assumes a darker colour than with platina alone.

Exp. 9. Recent muriate of tin is the most delicate test of mercury. It does not indicate the mercury in a mixed solution of that metal and platina.

From all these experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic state by green sulphate of iron. By *Experiments* 1 and 2, it is proved, 1st, That platina can protect a considerable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increase the action of nitromuriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in such a manner as not to be separated by the degree of heat necessary to fuse the compound, since after the fusion it retains that property, which is essentially characteristic of the presence of mercury in a solution of platina. The 8th *Experiment* proves that the action of mercury upon platina is not confined to the metallic state; but that these metals can combine and form an insoluble triple salt with an acid which produces a very soluble compound with platina alone. The 9th *Experiment* shows that platina can retain in solution a certain quantity of mercury, and prevent its reduction by a substance which acts most powerfully to that effect, when platina is not present. That part of the general position therefore which is the object of this paper is proved, if these experiments, upon being repeated by other chemists, shall be found to be accurate.

Hence, 1, 2, Platina protects mercury from nitric acid; and mercury renders platina more subject to nitro-mur. acid: 3—8. Platina retains mercury in the strong heat of fusion. 8. Mercury and platina act on each other in saline composition. 9. Platina defends mercury in solution from being reduced by m. of tin.

One or two of the above experiments seem to be in contradiction to some that I have stated in my paper upon palladium; for

Remark on palladium.

for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but confining myself to my present object, I shall wave all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those, which produce in any substance the greatest deviation from its usual properties.

The compounds of platina and mercury are subject to great variations, &c.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is dissolved in nitro-muriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed, than can determine the precipitation of the entire quantity of platina; yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

Investigation of the quantity of mercury thus fixed. It is supposed or inferred to be about 17 mercury to 83 platina, with the specific gravity 16.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and

not

not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury, and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that either my own observations, or those of others have given me cause to alter my opinion; I will add, that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results, yet the only means which I can, after all, prescribe for succeeding, is perseverance.

The author continues to think that palladium is a compound, since four successful experiments were made of forming it.

To ascertain whether the opinion of Mess. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

Platina, purified in Fourcroy and Vauquelin's method, is equally combinable with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Mess. Vauquelin and Fourcroy have promised the Institute of France a continuation of their researches, and M. Richter concludes his paper with saying that he will return to the subject. From the labours of such persons some great and important fact must issue, and I hope that the present subject will not be excluded from their consideration. The facts contained in this paper cannot be

These chemists and M. Richter have promised to pursue the subject.

 submitted

submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail unless proper precautions are used: that I have never operated upon less than one hundred grains; and that the results, which I have stated, however simple they may appear, have been the constant labour of some weeks.

POSTSCRIPT.

Dr. Wollaston's palladium in crude platina proves nothing; and it may have been the product of the amalgamating process.

Since this paper was written Dr. Wollaston has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollaston. I have not yet seen a copy of his paper; but I shall merely observe here that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events, the amalgamation to which platina is submitted before it reaches Europe, is sufficient to account for a small portion of palladium.

VI.

Method of obviating the Necessity of Lifting Ships. By Mr. ROBERT SEPPINGS, *of Chatham Yard* *.

Great saving and advantage of suspending instead of lifting ships.

THE method here to be described of suspending, instead of lifting, ships, for the purpose of clearing them from their blocks; affords a very great saving to the public; and abridges two-thirds of the time formerly used in this operation. From the saving of time another very important advantage is derived, namely, that of enabling large ships to be docked, suspended, and undocked, the same spring tides. Without enumerating the inconveniencies arising, and, perhaps, injuries, which ships are liable to sustain, from the former

* From the Transactions of the Society of Arts, who voted him the gold medal, 1804.

practice

practice of lifting them, and which are removed by the present plan; that which relates to manual labour deserves particular attention; twenty men being sufficient to suspend a first rate, whereas it would require upwards of 500 to lift her. The situation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the shoring and lifting of ships, as well as the other practical part of the profession of a shipwright. Here he had an opportunity of observing, and indeed it was a subject of general regret, how much time, expense, and labour, were required in lifting a ship, particularly ships of the line. This induced him to consider whether some contrivance could not be adopted to obviate these evils. And it occurred to him, that if he could so construct the blocks on which the ship rests, that the weight of the ship might be applied to assist in the operation, he should accomplish this very desireable end. In September 1800, the shoring and lifting the San Josef, a large Spanish first-rate, then in dock at Plymouth, was committed to his directions; to perform which, the assistance of the principal part of the artificers of the yard was requisite. In conducting this business, the plan, which will be hereafter described, occured to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, constructed by him, upon which the ship rests, being so contrived, that the facility in removing them, is proportionate to the quantity of pressure; and this circumstance is always absolutely under command, by increasing or diminishing the angle of three wedges, which constitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of consequence more acute; and its power may be so increased, that it shall have a great tendency to displace the horizontal wedges, as was proved by a model, which accompanied the statement to the Society; where the power of the screw is used as a substitute for the pressure of the ship.

Twenty men supply the place of 500.

History.

The contrivance is a block, compounded of two horizontal wedges, acting under an obtuse vertical wedge, which by the ship's weight, can, if needful, displace the others.

Mr. Seppings caused three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the first block were nine degrees; of the second, seven; and of the third, five; of course, the angle of the vertical wedge of the first block was 162 degrees; of the second 166; and of the third, 170 These blocks, or wedges,

Experiments.

Angles of the wedges.

wedges, were well executed, and rubbed over with soft soap for the purpose of experiment. They were then placed in a dock, in his Majesty's yard at Plymouth, in which a sloop of war was to be docked; on examining them after the vessel was in, and the water gone, they were all found to have kept their situations, as placed before the ship rested upon them. Shores in their wake were then erected to sustain the ship, prior to the said blocks being taken from under the keel. The process of clearing them was, by applying the power of battering-rams to the sides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were disengaged. It was observed, even in this small ship, that the block which was formed of horizontal wedges of nine degrees, came away much easier than those of seven, and the one of seven, than that of five. In removing the aforesaid blocks by the power of the battering-rams, which were suspended in the hands of the men employed, by their holding ropes passed through holes for that purpose, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to support the weight of the battering-rams, as well as to set them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams, which strikes the wedges. This was done before the blocks were again placed; and it has since been found fully to answer the purpose intended, particularly in returning the horizontal wedges to their original situations, when the work is performed for which they were displaced; the wheels also giving a great increase power to the rams, and decrease of labour to the artificers; besides which, the blows are given with much more exactness. The same blocks were again laid in another dock, in which a two-decked ship of the line was docked. On examination they were found to be very severely pressed, but were removed with great ease. They were again placed in another dock, in which a three-decked ship of the line was docked. This ship having in her fore-mast and bowsprit, the blocks were put quite forward, that being the part which presses them with the greatest force. As soon as the water was out of the dock, it was observed, that the horizontal wedges of nine and seven degrees had receded some feet from their original situations. This afforded Mr. Seppings a satisfactory proof, which experience has since demonstrated, (though

A sloop rested upon them, and shored up.

The horizontal wedges were then driven out,

by battering-rams,

supported on wheels.

Other experiments.

It was found that the weight of the vessel was capable of pressing out the wedges;—

(though many persons before would not admit of, and others could not understand, the principle) that the facility of removing the blocks or wedges, was proportionate to the quantity of pressure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the sides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, farther corroborated by the testimonials of his then superior officers, was so satisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of cast iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any pressure, having in no instance receded, and, when required, were easily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron three quarters of an inch thick, so that iron at all times acts in contact with iron.

and therefore that if made of the proper angle they would be secure, and always capable of being driven out.

The best angle is about $5\frac{1}{2}$ degrees for cast iron wedges.

The placing the sustaining shores, the form and sizes of the wedges, and battering-rams, &c. also the process of taking away, and again re-placing, the wedges of which the block is composed, are also exemplified by a model.

The dock being prepared at Plymouth, in August, 1801, the Canopus, a large French 80-gun ship, was taken in, and rested upon the blocks; and the complete success of the experiment was such, that other docks were ordered to be fitted at Sheerness and Portsmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and at the latter a three-decked ship, were suspended in like manner. This happened in December, 1802, and January, 1803; and the reports were so favourable, as to cause directions to be given for the general adoption of these blocks in his Majesty's yards. This invention being thought of national consequence, with respect to ships, but particularly those of the navy, government has been pleased to notice and reward Mr. Seppings for it.

A large 80-gun ship suspended by this method.

The time required to disengage each block, is from one to three minutes after the shores are placed: and a first-rate fits on

Each block may be disengaged in three minutes.

on about fifty blocks. Various are the causes for which a ship may be required to be cleared from her blocks, viz. to shift the main keel; to add additional false keel; to repair defects; to caulk the garboard seams, scarples of the keel, &c. Imperfections in the false keel, which are so very injurious to the cables, can in the largest ship be remedied in a few hours by this invention, without adding an additional shore, by taking away blocks forward, amid-ships, and abaft, at the same time; and when the keel is repaired in the wake of those blocks, by returning them into their places, and then by taking out the next, and so on in succession. The blocks can be replaced in their original situations, by the application of the wheel battering-rams to the wedges, the power of which is so very great, that the weight of the ship can be taken from the shores that were placed to sustain her. There were one hundred and six ships of different classes, lifted at Plymouth dock-yard, from the 1st of January, 1798, to the 31st of December, 1800; and, had the operation of lifting taken less time, the number would have been very considerably increased; for the saving of a day is very frequently the cause of saving the spring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be sufficiently estimated.

It is not required to suspend the ship in all cases. For the repairs may be done by successive removal of blocks.

The lifting of ships was a frequent operation in the navy.

This invention may be applied with great advantage, whenever it is necessary to erect shores, to support any great weights, as, for instance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his Majesty's ship Glory, of 98 guns, used wedges of Mr. Seppings's invention for a fid of a top-gallant mast of that ship. In 1803, the top-gallant masts of the Defence, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, since she has been cruizing in the North Sea, the wedge fids have been found in every respect to answer.

This invention is of value in other undertakings.

Fid of a top gallant mast,

But it is Mr. Seppings's wish that it should be understood, that the idea of applying this invention to the fid of a top-gallant mast originated with Capt. Wells, who well understood the principle, and had received from him a model of the invention.

applied by Capt. Wells.

When it is required to strike a top-gallant mast, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place, is taken out, by one man going aloft for that purpose; the other horizontal wedge is worked in the

Manœuvre of striking the mast, &c.

fid

fid, as shown in the drawing and model that accompany this statement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant masts in this way is, that they can be struck at the shortest notice, and without slacking the rigging, which is frequently the cause of springing and carrying them away, particularly those with long pole heads. The angle of the horizontal wedges for the fids of masts should be about twenty degrees.

It can now be done without slacking the rigging.

The above Account was accompanied with Certificates from Sir John Henslow, Surveyor of the Navy; Mr. M. Didram, master-shipwright of Portsmouth-Yard; and Mr. John Carpenter, foreman of Sheerness Dock-yard, confirming Mr. Seppings's statement.

Certificates.

Reference to the Engraving of Mr. Seppings's method of obviating the necessity of lifting Ships. Plate XI.

This plan and section of a seventy-four gun ship describes the method of obviating the necessity of lifting ships, when there may be occasion to put additional false keels to them, or to make good the imperfections of those already on; also when it may be necessary to caulk the garboard seams, scarples the keel, &c. by which means a very considerable part of the expense will be saved, and much time gained. The blocks are cleared, and again returned by the following process. A sufficient number of shores are placed under the ship to sustain her weight, and set taught, stationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any opposite the blocks, as they would in that case hinder the return of the wedges with the battering-rams. A blow must then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cause them to slide aft, as shown in the plan. The battering rams abaft then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great ease cleared, and the angular block on the top will drop down. When the work is performed, the block must be replaced under the keel, and the wedges driven back by working the rams athwart-ships, as described in the section.

Description and reference to the plate, &c.

Instructions.

N. B. In

N. B. In returning the iron wedges, to avoid ſtraining the angular blocks, it is propoſed to leave a few of them out forward and aft, and ſtop the ſhip up, by laying one iron wedge on the other, as ſhown at Fig. 1, Plate XI.

To facilitate the buſineſs, blocks may be cleared forward and aft at the ſame time, ſufficient to get in place one length of falſe keel. If the falſe keel ſhould want repairing, it may be done without any additional ſhores, by clearing one block at a time, and when the keel is repaired in the wake of that block, return the wedges, as above directed, and clear the next, &c.

Section and Plan, Plate XI. *Fig.* 2.

Parts of the ſection and plan.

A. Keelſon.
B. Ceiling.
C. Floor timber.
D. Dead or riſing wood.
E. Plank of the bottom.
F. Keel and falſe keel.
G. Angular blocks with a half-inch iron-plate bolted to them.
H. Caſt-iron wedges.
I. Iron plate of three-fourths of an inch thick on the bottom of the dock.
K. Battering-rams, with wheels, and ropes for the hands.
L. Caſt-iron wedges, having received a blow from forward.
M. Shores under the ſhip to ſuſtain her weight.

Fig. 3, repreſents part of a top-gallant maſt fitted with a wedge fid.
a. Top-gallant maſt.
b. Fid, with one horizontal wedge worked on it.
c. Moveable wedge, with the iron ſtrap and pin over it, to keep it in its ſituation.
d. Truſſel trees.

VII. On

VII.

On muſcular Motion. *By* ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1804.

ANIMAL phyſiology has derived ſeveral illuſtrations and additions, from the inſtitution of this lecture on muſcular motion; and the details of anatomical knowledge have been conſiderably augmented by deſcriptions of muſcular parts before unknown. Introduction.

Still, however, many of the phenomena of muſcles remain unexplained, nor is it to be expected that any ſudden inſulated diſcovery ſhall ſolve ſuch a variety of complicated appearances.

Muſcular motion is the firſt ſenſible operation of animal life: the various combinations of it ſuſtain and carry on the multiplied functions of the largeſt animals: the temporary ceſſation of this motive faculty is the ſuſpenſion of the living powers, its total quieſcence is death. Muſcular motion.

By the continuance of patient, well directed reſearches, it is reaſonable to expect much important evidence on this ſubject and, from the improved ſtate of collateral branches of knowledge, together with the addition of new ſources, and methods of inveſtigation, it may not be unreaſonable to hope for an ultimate ſolution of theſe phenomena, no leſs complete, and conſiſtent, than that of any other deſideratum in phyſical ſcience.

The preſent attempt to forward ſuch deſigns is limited to circumſtances which are connected with muſcular motion, conſidered as cauſes, or rather as a ſeries of events, all of which contribute, more or leſs, as conveniencies, or eſſential requiſites, to the phenomena; the details of muſcular applications being diſtinct from the objects of this lecture.

No ſatisfactory explanation has yet been given of the ſtate or changes which obtain in muſcles during their contractions or relaxations, neither are their correſponding connections with the vaſcular, reſpiratory, and nervous ſyſtems, ſufficiently traced. Theſe ſubjects are therefore open for the preſent enquiry, and, although I may totally fail in this attempt to elucidate

Neither the changes in muſcles during action, nor their connections with the other parts of the ſyſtem have been explained.

cidate any one of the ſubjects propoſed, nevertheleſs I ſhall not eſteem my labour uſeleſs, or the time of the Royal Society altogether unprofitably conſumed, if I ſucceed in pointing out the way to the future attainment of knowledge ſo deeply intereſting to mankind.

The muſcle itſelf is fibrous.

The muſcular parts of animals are moſt frequently compoſed of many ſubſtances, in addition to thoſe which are purely muſcular. In this groſs ſtate, they conſtitute a flexible, compreſſible ſolid, whoſe texture is generally fibrous, the fibres being compacted into faſciculi, or bundles of various thickneſs. Theſe fibres are elaſtic during the contracted ſtate of muſcles after death, being capable of extenſion to more than one-fifth of their length, and of returning again to their former ſtate of contraction.

The enveloping membrane is elaſtic.

This elaſticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be ſafely aſſumed that the intrinſic matter of muſcle is not elaſtic.

The attraction of coheſion, in the parts of muſcle, is ſtrongeſt in the direction of the fibres, it being double that of the contrary, or tranſverſe direction.

Muſcles are irritable during life.

When muſcles are capable of reiterated contractions and relaxations, they are ſaid to be alive, or to poſſeſs irritability. This quality fits the organ for its functions. Irritability will be conſidered, throughout the preſent lecture, as a quality only.

They have leſs coheſion lengthways when dead.

When muſcles have ceaſed to be irritable, their coheſive attraction in the direction of their fibres is diminiſhed, but it remains unaltered in the tranſverſe direction.

Experiment in proof.

The hinder limbs of a frog attached to the pelvis being ſtripped of the ſkin, one of them was immerſed in water at 115° of Fahrenheit, during two minutes, when it ceaſed to be irritable. The thigh bones were broken in the middle, without injuring the muſcles, and a ſcale affixed to the ancle of each limb: a tape paſſed between the thighs was employed to ſuſpend the apparatus. Weights were gradually introduced into each ſcale, until, with five pounds avoirdupois, the dead thigh was ruptured acroſs the fleſhy bellies of its muſcles.

repeated.

The irritable thigh ſuſtained ſix pounds weight avoirdupois, and was ruptured in the ſame manner. This experiment was repeated on other frogs, where one limb had been killed by a watery ſolution of opium, and on another where eſſential oil of

of cherry laurel * was employed: in each experiment, the irritable limb sustained a weight one-sixth heavier than the dead limb.

It may be remarked, in confirmation of these experiments, that when muscles act more powerfully, or more rapidly, than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the instances of ruptured tendo achillis, and fractured patella. Instances have however occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence, a fact which has been always assumed by anatomists from the determinate forces which muscles exert. The same doctrine confirmed by effects in the living subject.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual. Differences observable in the colour, texture, &c. of muscular parts.

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red; the amphibia, the accipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game †, have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness, and the reticular membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

An example of the origin of muscle is presented in the history of the incubated egg, but whether the rudiments of the punctum saliens be part of the cicatricula organised by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular Origin of muscle in the egg. Punctum saliens

* Distilled oil from the leaves of the *Prunus Laurocerassus.*

† *Tetrao tetrix.* Lin.

confirmation

4

confirmation of which would improve the knowledge of animal generation by shewing that it is gemmiferous. There are sufficient analogies of this kind in nature, if reasoning from analogies were proper for the present occasion.

The punctum saliens, during its first actions, is not encompassed by any fibres discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards and backwards, in the same vessels. The commencement of life in animals of complex structure is, from the preceding fact, like the ultimate organization of the simpler classes.

Muscles of birds are formed out of the albumen, with a small portion of vitellus and atmos. fluid,

It is obvious that the muscles of birds are formed out of the albumen ovi, the vitellus, and the atmospheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly consumed during incubation, and the vitellus little diminished, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muscular parts, which constitute the greater proportion of such animals when hatched, are made out of the albumen, a small portion of the vitellus, and certain elements, or small quantities of the whole compound of the atmosphere.

and they do not differ from those of the mammalia.

The muscles of birds are not different, in any respect, from those of quadrupeds of the class of mammalia.

The anatomical structure of muscular fibres is generally complex, as those fibres are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

Muscular fibre is cylindric; membranous without, and pulpy within.

A muscular fibre, duly prepared by washing away the adhering extraneous substances, and exposed to view in a powerful microscope, is undoubtedly a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated, and of little cohesive power when dead.

The ultimate fibres.

A difficulty has often subsisted among anatomists concerning the ultimate fibres of muscles; and, because of their tenuity, some persons have considered them infinitely divisible, a position which may be contradicted at any time, by an hour's labour at the microscope.

Arteries.

The arteries arboresce copiously upon the reticular coat of the muscular fibre, and in warm-blooded animals these vessels are

are of fufficient capacity to admit the red particles of blood, but the intrinfic matter of mufcle, contained within the ultimate cylinder, has no red particles.

The arteries of mufcles anaftomofe with correfponding veins; but this courfe of a continuous canal cannot be fuppofed to act in a direct manner upon the matter of mufcle.

The capillary arteries terminating in the mufcular fibre muft alone effect all the changes of increafe in the bulk, or number, of fibres, in the replenifhment of exhaufted materials, and in the repair of injuries; fome of thefe neceffities may be fuppofed to be continually operating, It is well known, that the circulation of the blood is not effential to mufcular action; fo that the mode of diftribution of the blood veffels, and the differences in their fize, or number, as applied to mufcles, can only be adaptations to fome fpecial convenience.

Vafcularity is not infinite, but perceptibly limited.

Another prevalent opinion among anatomifts, is the infinite extenfion of vafcularity, which is contradicted in a direct manner by comparative refearches. The feveral parts of a quadruped are fenfibly more or lefs vafcular, and of different contextures; and, admitting that the varied diameter of the blood veffels difpofed in each fpecies of fubftance, were to be conftituted by the grofs fenfible differences of their larger veffels only, yet, if the ultimate veffels were in all cafes equally numerous, then the fole remaining caufe of diffimilarity would be in the compacting of the veffels. The vafa vaforum of the larger trunks furnifh no reafon, excepting that of a loofe analogy, for the fuppofition of vafa vaforum extended without limits. Moreover, the circulating fluids of all animals are compofed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows that the veffels through which fuch fluids are to pafs, muft be of fufficient capacity for the fize of the particles, and that fmaller veffels could only filtrate water devoid of fuch animal particles: a pofition repugnant to all the known facts of the circulation of blood, and the animal economy.

Capillary arteries,

The capillary arteries which terminate in the mufcular fibre, muft be fecretory veffels for depofiting the mufcular matter, the lymphæducts ferving to remove the fuperfluous extravafated watery fluids, and the decayed fubftances which are unfit for ufe.

 The

and lymphæducts.

The lymphæducts are not so numerous as the blood vessels, and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the intersticial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa, and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

Nerves of voluntary muscles.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles, than to any other substances, the organs of the senses excepted.

Origin of the nerves of volition.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the Cerebrum and Cerebellum, and from the extension of that substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opaque medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these junctions,

Muscles for involuntary motion.

junctions, called Ganglions, and which are composed of a less proportion of the medullary substance, and their texture is firmer than that of ordinary nerves. Ganglions.

The terminal extremities of nerves have been usually considered of unlimited extension; by accurate dissection however, and the aid of magnifying glasses, the extreme fibrils of nerves are easily traced as far as their sensible properties, and their continuity extends. The fibrils cease to be subdivided whilst perfectly visible to the naked eye, in the voluntary muscles of large animals, and the spaces they occupy upon superficies where they seem to end, leave a remarkable excess of parts unoccupied by those fibrils. The extreme fibrils of nerves lose their opacity, the medullary substance appears soft and transparent, the enveloping membrane becomes pellucid, and the whole fibril is destitute of the tenacity necessary to preserve its own distinctness; it seems to be diffused and mingled with the substances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary sensation, appear to be in the reticular membrane, the common covering of all the different substances in an animal body, and the connecting medium of all dissimilar parts. The extremities of nerves are definite in their termination.

By this simple disposition, the medullary substance of nerve is spread through all organized, sensible, or motive parts, forming a continuity which is probably the occasion of sympathy. Peculiar nerves, such as the first and second pairs, and the portio mollis of the seventh, terminate in an expanse of medullary substance which combines with other parts and membranes, still keeping the sensible excess of the peculiar medullary matter. Diffusion of nervous matter.

The peculiar substance of nerves must in time become inefficient; and, as it is liable to injuries, the powers of restoration, and repair, are extended to that material. The re-union of nerves after their division, and the reproduction after part of a nerve has been cut away, have been established by decisive experiments. Whether there is any new medullary substance employed to fill up the break, and, if so, whether the new substance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the history of the formation of a fœtus, the structure of certain monsters, and the organization of simple animals, all seem to Restoration or repair of nerves.

 favour

favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal seat of the cerebral medulla.

Whether the matter of nerve be not extensively mixed in all irritable parts.

This doctrine, clearly established, would lead to the belief of a very extended commixture of this peculiar matter in all the sensible and irritable parts of animals, leaving the nerves in their limited distribution, the simple office of conveying impressions from the two sentient masses with which their extremities are connected. The most simple animals in whom no visible appearances of brain or nerves are to be found, and no fibrous arrangement of muscles, may be considered of this description: Mr. John Hunter appeared to have had some incomplete notions upon this subject, which may be gathered from his representation of a materia vitæ in his Treatise on the Blood, &c. Perhaps it would be more proper to distinguish the peculiar matter of muscle by some specific term, such, for example, as materia contractilis.

Peculiar adaptation for the nerves of electric animals.

A particular adaptation for the nerves which supply the electrical batteries of the torpedo, and gymnotus, is observable on the exit of each from the skull; over which there is a firm cartilage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression: so that a voluntary muscle probably governs the operations of the battery.

The matter of the nerves, and brain, is very similar in all the different classes of animals.

The external configuration of animals is not more varied than their internal structure.

Configuration and structure of the various classes of animals.

The bulk of an animal, the limitation of its existence, the medium in which it lives, and the habits it is destined to pursue, are each, and all of them, so many indications of the complexity or simplicity of their internal structure. It is notorious that the number of organs, and of members, is varied in all the different classes of animals; the vascular and nervous systems, the respiratory, and digestive organs, the parts for procreation, and the instruments of motion, are severally varied, and adapted to the condition of the species. This modification of anatomical structure is extended in the lowest tribes of animals, until the body appears to be one homogeneous substance.

Very simple animals.

The cavity for receiving the food is indifferently the internal, or external surface, for they may be inverted, and still continue

tinue to digeſt food; the limbs or tantacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally ſenſible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the reſpiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilſt neither blood-veſſels, nerves, nor muſcular fibres, are diſcoverable by any of the modes of inveſtigation hitherto inſtituted.

From this abſtract animal (if ſuch a term may be admitted) up to the human frame, the variety of acceſſory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of deſign, and of accommodations to the purpoſes which fix the order of nature. In all animal ſtructures deſign is evident.

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of ſome liable to be diſpenſed with. The water transfuſed throughout the interſticial ſpaces of the animal fabric, the combinations with lime in bones, ſhells, and teeth; the horns, hoofs, ſpines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their ſubſtance, extra-vaſcular, inſenſible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extenſive obſervation, that many more parts of animal bodies may be conſidered as inanimate ſubſtances; even the reticular membrane itſelf ſeems to be of this claſs, and tendons, which may be the condenſed ſtate of it; but theſe particulars are foreign to the preſent occaſion. In the more complicated animals much of their materials are not alive;

The deduction now to be made, and applied to the hiſtory of muſcular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adheſions of the muſcles of multi-valve, and bi-valve ſhell fiſh, to the inorganic ſhell, the cancer Bernhardus to the dead ſhells of other animals, and in the tranſplantation of teeth. All of which, although ſomewhat contrary to received opinion, have certainly no degree of vaſcularity, or vital connection with the inhabitant; theſe ſhells being liable to tranſudations of cupreous ſalts and other poiſonous ſubſtances, whilſt the animal remains uninjured. A variety of proofs to the ſame effect ſo that animated matter may be connected with inanimate.

effect might be adduced, but it would be difrefpectful to to this learned body to urge any farther illuftrations on a fubject fo obvious.

Divifion of the parts of an animal deftroys the conformation;

The effects of fubdivifion, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter inftance, the entire properties of the fubftance are retained, however extenfive the fubdivifion; in the former fubftances, the comminution of parts deftroys the effential texture and compofition, by feparating the grofs arrangements of ftructure upon which their fpecific properties depend. From fimilar caufes it feems to arife, that animals of minute bulk are neceffarily of fimple ftructure: fize alone is not, however, the fole caufe of their fimple organization, becaufe examples are fufficiently numerous wherein the animal attains confiderable bulk, and is of fimple ftructure, and *vice verfâ*: but, in the former, the medium in which they live, and the habits they affume, are fuch as do not require extenfive appendages, whilft the fmaller complex animals are deftined to more difficult, and more active exertions. It may be affumed however, as an invariable pofition, that the minuteft animals are all of fimple organization.

but lefs the more fimple the ftructure,

Life may on a fmall fcale be fupported with fimple materials; bulky animals require variety of organs.

Upon a fmall fcale, life may be carried on with fimple materials; but the management, and provifions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by fimple apparatus; thus, the fkeleton which gives a determinate figure to the fpecies, fupports its foft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being fufficiently ftrong, and yet light enough for the moving powers; but the fkeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which laft cafe the bulk is not an abfolute caufe. The examples of teftaceous vermes, and coleopterous, as well as moft other infects, are univerfally known.

Thus large animals have their bones within them, fmaller have them without.

Cryftalline of the eye, mufcular.

The opinion of the mufcularity of the cryftalline lens of the eye, fo ingenioufly urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of mufcle, from the centre to the circumference of the lens, and thefe compacted into angular maffes, would produce fpecific alterations in its figure.

This

This rapid ſketch of the hiſtory of muſcular ſtructure has been obtruded before the Royal Society to introduce the principal experiments, and reaſonings which are to follow: they are not ordered with ſo much exactneſs as becomes a more deliberate eſſay, but the intention already ſtated, and the limits of a lecture are offered as the apology.

Temperature has an eſſential influence over the actions of muſcles, but it is not neceſſary that the ſame temperature ſhould ſubſiſt in all muſcles during their actions; neither is it eſſential that all the muſcular parts of the ſame animal ſhould be of uniform temperatures for the due performance of the motive functions. Temperature of muſcles.

It appears that all the claſſes of animals are endowed with ſome power of producing thermometrical heat, ſince it has been ſo eſtabliſhed in the amphibia, pices, vermes, and inſecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in theſe examples, ſufficient to preſerve their equable temperature in cold climates, ſo that they yield to the changes of the atmoſphere, or the medium in which they reſide, and moſt of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, poſſeſs only a power of reſiſting certain limited degrees of cold; and their ſurfaces, as well as their limbs, being diſtant from the heart, and principal blood-veſſels, the muſcular parts ſo ſituated are ſubject to conſiderable variations in their temperature, the influence of which is known. All animals do produce heat; but are alſo affected by external communication.

In thoſe claſſes of animals which have little power of generating heat, there are remarkable differences in the ſtructure of their lungs, and in the compoſition of their blood, from the mammalia and aves. The colder animals.

Reſpiration is one of the known cauſes which influences the temperatures of animals: where theſe organs are extenſive, the reſpirations are performed at regular intervals, and are not governed by the will, the whole maſs of blood being expoſed to the atmoſphere in each circulation. In all ſuch animals living without the tropics, their temperature ranges above the ordinary heat of the atmoſphere, their blood contains more of the red particles than in the other claſſes, and their muſcular irritability ceaſes more rapidly after violent death. Reſpiration: its organs in animals of higher temperature;

The

and in the cold-blooded:

The respirations of the animals denominated "cold-blooded," are effected differently from those of high temperature; in some of them, as the amphibia of Linnæus, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water, but whether the water alone, or the atmospheric air mingled with it, furnish the changes in the pulmonary blood, is not known.

in insects.

In most of the genera of insects, the lungs are oborescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs, but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals, the blood contains a smaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

Experiment: Cold blooded animals were included in water over mercury. After some days they died; but no gas was emitted, nor was the water changed.

The following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver; *viz.* one gold fish, one frog, two leeches, and one fresh-water muscle.* These animals were confined for several days, and exposed in the sun in the day-time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh-water muscle on the thirteenth, This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

Hybernating animals can live under confined respiration.

Animals of the class mammalia which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins; the superior cava divides into two trunks; the left, passing over the left auricle of the heart, opens into

Peculiarity of structure in the heart and its veins.

* *Mytilus Anatinus.*

the

the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins usually called azygos, accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

Their habit and circumstances that accompany the torpid state, &c.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera, and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the longitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the last weak actions of life, and to its gradual recommencement.

The manner in which that state commences and is afterwards terminated.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed, coldness and insensibility take place, and finally the heart concludes its motions, and the muscles cease to be irritable. It is worthy of remark that a confined air, and a confined respiration, ever precede these phenomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen, without the destruction of the muscular irritability, and this always happens to the garden snail*, and to the chrysalides of many insects during the winter of this climate.

(The conclusion in our next.)

* *Helix nemoralis.*

Description

VIII.

Description of a Boring Tube, in general Use in America; but less known in this Country.

Description of the American borer.

FIG. 2. *Plate* X. exhibits a very simple and ingenious borer, consisting of the common center bit of the carpenters followed by a wide flat thread screw, hammered up from a plate of iron or steel. It is said that they are used to bore holes several feet in length, and the peculiar property possessed by this instrument is, that it clears the cutting without requiring to be drawn out, as is the case with the augur, the gimblet, and other similar tools. I do not, however, think that it would have this effect in boring perpendicularly down to considerable depths; but for horizontal or slightly inclined holes, its effect must fully answer.

Popular explanation of its action.

It may not at first occur to the reader why the introduction of this tool into a hole which must contain the wood that formerly blocked it up, should not be attended with some degree of impediment or jamming; but this difficulty will vanish, when it is considered that the cuttings are, partly by their weight, and partly by friction against the internal cylindrical surface, prevented from revolving along with the screw. The consequence is that they are pressed against its thread, and slide along it towards the handle. And as this motion or shifting of the thread is quicker than the motion of boring, by which the whole tool is carried inwards, the cuttings must come out with a velocity nearly equal to the difference of these two motions.

IX.

Geographical and Topographical Improvements. By JOHN CHURCHMAN, *Esq. M. Imp. Acad. of Sciences, Petersburgh.**

Great value of topographical knowledge.

IT appears to be a matter of much importance to the people of any country, at all times, whether in war or peace; to pos-

* From the Transf. of the Soc. of Arts for 1804, who voted the silver medal to him for the same.

sess

ſeſs a complete knowledge of its ſurface. In war, ſuch knowledge is abſolutely neceſſary for defence; in peace, for improving the country to the beſt advantage.

Now, ſince geography may be improved, an eaſy and accurate method to lay down maps of mountainous countries and hilly eſtates, will perhaps prove uſeful, as it will ſhow at a ſingle view the true ſhape and comparative height of the ground without the art of painting.

Utility of a correct delineation of hills, &c.

As mountains are apt to eclipſe each other, a perſpective view is ſeldom very extenſive, the rules of which fall ſhort of giving an accurate idea of any hilly country; becauſe ſuch a view, though ſtrictly true in one particular place, is not ſo in any other. The altitudes of mountains appear in proportion to the diſtance from the eye, and no rule in geometry has been found ſufficient to determine diſtances from any ſingle ſtation. Neither can a bird's-eye view of an eſtate aſcertain the depth of valleys or the height of mountains. But the method here propoſed will be found equally capable of giving the true ſhape of any ground above or below water. It may be ſucceſsfully applied to ſea charts, and will prevent much confuſion, ariſing from the tedious method of diſtinguiſhing ſoundings by a multitude of figures.

Mountains cannot be uſefully ſhewn in perſpective.

Explanation.

Suppoſe a full deſcription is required of any iſland in the ocean. Firſt, let an accurate map be laid down in the common way; and let the perpendicular height between the higheſt point of land and the ocean be divided into any number of equal parts. Suppoſe theſe equal diviſions are 100, 200, 300, 400 feet above the low-water mark. From the different points of theſe ſeveral diviſions, let horizontal lines be run with a good theodolite, and ſpirit level annexed, all round the iſland. If the work is well done, each line will end where it began; and if the bearings and diſtances of theſe ſeveral lines are truly laid down on the map, the crooked courſes of them will clearly ſhow the ſhape of the ground over which they paſs. For example: if any horizontal line paſſes by the ſide of a ſteep hill, it will incline towards the ocean, or approach the next horizontal line below it. When the ſame line croſſes a ſtream of running water or a valley, it will naturally bend up the ſide of the ſaid ſtream, until it can croſs it without loſing the

New method. Make a good map or plan. Mark the point of higheſt elevation and other points differing in elevation by equal meaſures. Run lines with the theodolite and level from theſe points; in which if the ſurvey be good, they will again terminate. Theſe lines laid on the map will ſhew the figure of the country.

the level; or, in other words, it will bend towards the centre of the island. Hence, by a little practice, the shape of the several horizontal lines on the map will give as clear an idea to the mind, of the shape of any country over which they pass, as a sight of the country itself can convey to the eye. But to obtain a mathematical and true knowledge of the altitude and declivity of any part of the country, we have the following proposition:

Trigonometrical rule for the declivities.

As the perpendicular height of any one horizontal line above another is to the radius: so is the horizontal distance between the horizontal lines measured on the map at any particular place: to the co-tangent of declivity at that place.

Note.—If the horizontal distance between any two horizontal lines on the map is equal to the perpendicular height of any horizontal line above another, the angle of altitude, or declivity, of any hill will be 45 degrees.

Advantages;

The present improvement, which I believe to be entirely new, will be found to possess the following advantages:

—to military men,

1st. Military men are well acquainted with the many advantages always to be gained from the exact representation of high grounds. By this method, we are able to give the angle of altitude, the angle of declivity, and perpendicular height of every hill; likewise the comparative height of different hills, the best route by which the high grounds may be gradually ascended, and where heavy burthens can be drawn up with most ease.

—and for domestic and economical purposes.

2dly. Experience has sufficiently shown, that the inhabitants of low grounds are subject to different kinds of sickness, from which those living at places elevated to a certain degree are exempt. A map on this improved plan will point out the most proper situation for building dwelling-houses. It will be useful in botany, in discovering or cultivating some kinds of plants which flourish best at particular distances above the level of the ocean. It will trace the line of vegetation on the sides of lofty mountains, whose tops are covered with eternal snow.

—to direct agricultural improvements,

3dly. Some high lands are known to produce good grain, while low lands afford grass more abundantly; but most grounds produce good grass, over which a moderate quantity of running water is conveyed. A plan of any country in this way will show all the ground that can be irrigated; where

water-

water-works may be erected; where navigable canals may be cut; and where high-ways and rail-roads may be laid out on the best and most level ground.

4thly. The subterraneous treasures of the mineral and fossil kingdoms are generally found in strata; and if they are not truly horizontal, they make a certain angle with the horizon. A map on this projection may enable the mineralogist to follow any one stratum, at places even far distant from each other. —and mine researches.

Problem.

To find the true declivity of any piece of ground, in any map laid down on the principles of the present plan. Examples computed.

Example 1st. for D. *see Plate* IV.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal distance, 4 feet -	60206
	10.60206
To the co-tangent of the declivity, 45° -	10.00000

Example 2d. for B.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal distance, 8 feet -	90309
	10.90309
To the co-tangent of the declivity, 26° 34′	10.30103

Example 3d. for C.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal distance, 18 feet	1.25527
	11.25527
To the co-tangent of the declivity, 12° 32′	10.65321

The annexed survey, *Plate* IX. of a small lake and artificial mountain in the garden of his Excellency Count de Strogonoff, near St. Petersburgh, has been closed by the tables of the difference of latitude and departure, as follows: A survey according to this method.

N 30 E

		N	S	E	W
N 30 E	2½	2.2	—	1.2	—
N 35 E	2	1.6	—	1.1	—
N 75 E	2	.5	—	1.9	—
N 55 E	2	1.1	—	1.6	—
N 45 E	3	2.1	—	2.1	—
N 52 W	2	1.2	—	—	1.6
N 59 W	3	1.5	—	—	2.5
S 56 W	12	—	6.7	—	9.9
S 60 E	7	—	3.5	6.1	—
		10.2	10.2	14.0	14.0

X.

A Memoir on the refining of Lead in the large Way. Containing some Reflections on the Inconveniencies resulting from Cupels made of Ashes; with a new and œconomical Method of constructing those Cupels. By CITIZEN DUHAMEL.*

Process for refining lead. Cupels made of ashes.

EVERY one knows that in order to effect the separation of silver from lead, a process called refining or cupellation is made use of, which is effected in a bason called a cupel; and it is likewise known that this bason is formed of the ashes or incinerated remains of animal or vegetable substances, after depriving them by washing of what saline matter they may contain.

The great quantity of wood ashes required for making these cupels, and the difficulty of procuring it, have long ago induced me to seek for a more simple and less expensive means of forming these vessels.

Early observation that the oxide of lead penetrates chemical vessels, while silver, if present, remains metallic.

The early chemists having observed that lead becomes oxided and converted into what is called litharge, when it is exposed

* This memoir is translated from the Memoirs of the French National Institute, Vol. III. at the request of a correspondent.

posed to fire with the contact of atmospheric air, while the silver it may contain, preserves its metallic form; it only remained for them to contrive a method of separating these two metals. They were led to this method by observing that the oxide of lead, in its state of liquefaction easily penetrates the substances with which it may be in contact, particular bone ashes, without destroying the figure of the vessel made of that material. In fact there is no substance whatever which is better adapted to form the small cupels for assaying.

For want of bone ashes the ashes of wood are used for cupels; they have many inconveniences.

The difficulty, and often the impossibility of procuring three or four bushels of bone ashes in Germany, for each time of refining, has led to the adoption of the ashes of wood. But not to mention that these are of considerable price, and not always to be had, they present another inconvenience, by often rising and floating on the surface of the lead. When this happens, the process must fail; and it does happen as often as the ashes are ill prepared, or the cupel not sufficiently or irregularly beaten, or that the openings left for the evaporation of the humidity are not properly disposed, or enough in number, or closed by a portion of the scoriæ upon which the floor is made to receive the ashes. This floor ought to be constructed of the most porous bricks, in order that the water with which the ashes must be wetted may penetrate them and evaporate into the bed of scoria, and escape through the opening at the bottom of the furnace.

The elasticity of this aqueous vapour frequently causes explosions, which not only disorder the cupel, but even the masonry of the furnace, if it be not properly constructed.

Cupelling of lead.

In order to ascertain the proportion of silver in any quantity of lead, it is only necessary to pass a few pennyweights into a small cupel of bone ashes placed under the muffle of an assayer's furnace; as the lead becomes oxided it is imbibed in the cupel, and at length the phenomenon of brightening takes place upon the metallic button. This appearance shews that all the lead is dissipated, and the remaining silver in a pure state.

Refining in the large way cannot be done by the assaying process wherein the oxide is absorbed.

In the large way of refining, the same object of separating the silver from the lead is aimed at, but the lead is not intended to penetrate into the cupel, which in fact is impracticable. For the total absorption of this metal would require a much greater quantity of ashes, with the consumption of a ten

fold portion of time and fuel; besides which, the loss in recovering the lead by fusion of the cupel is very expensive and the product less considerable than in the common way. The oxide of lead obtained in this last method may be easily fused, and reduced if needful; but it is an article of value in the arts, and therefore very acceptable in the market in its state of oxide.

Vessels of clay used in smelting lead.

The lead ores and litharge may be fused as is done in England and Britanny, in a reverbatory furnace, of which the floor or bason is formed of moistened and rammed clay. These floors resist the action of fire as well as that of the oxide of lead during six or eight months constant work.

These are applicable to refining.

The durability of these basons of earth first gave me a notion of the method I shall propose for refining furnaces, in which the intention is to oxide the lead in order to obtain litharge, and not to cause it to be totally absorbed in the cupels, as is done when the metal is assayed, to shew how much silver it contains.

It would be preferable that no absorption took place.

In the operation upon a large scale the cupel, though of ashes absorbs only part of the lead, as I have already remarked, observing at the same time that it would be much more advantageous to obtain the whole converted into litharge, of which the reduction into lead is infinitely more easy than that of the oxide contained in the ashes, which resist fusion, and afford a scoria that always contains some metal.

English process. 12 tons of lead are oxided with scarcely any absorption.

Upon a cupel of ashes rammed into an oval ring of iron, about five feet in length and three and a half in width, the English refine in succession about twelve ton of lead, which becomes converted into fine merchantable litharge, with the exception of the small portion that penetrates the cupel; of which the thickness is less than three inches. This cupel is supported under the roof of the furnace by two bars of iron. The litharge is driven by the blast of bellows towards the anterior part of the furnace, whence it falls without interruption upon the area of the foundery; and at the same time to supply the space which this subtraction of oxide would leave, a pig of lead is gradually advanced into the interior of the furnace, placed on one side of the nozel of the bellows. This lead, by its gradual fusion, keeps the cupel full till towards the end of the operation.

I have given this ſketch of the Engliſh proceſs only to ſhew that it is poſſible to refine lead with very little expence of aſhes for forming the cupels. Thoſe here mentioned do not abſorb eighty pounds of oxide out of the large quantity of lead thus refined.

The veſſel is very cheap.

Hence we ſee that metallurgiſts have always endeavoured to obtain the greateſt poſſible quantity of litharge and the leaſt of aſhes containing oxide; but as they did not imagine they could depart from the docimaſtic proceſs, they have conſtantly made their cupels of aſhes.

We have ſeen that in the ſmall proceſs of cupellation the lead penetrates the aſhes as it becomes oxided, and that when no more lead remains the ſmall button of ſilver remains pure at the bottom in the ſpherical form. This operation is effected with more ſpeed becauſe the ſurface of the bath is always convex in theſe ſmall veſſels, and conſequently the litharge runs off on all ſides towards the edge of the cupel, where it is immediately imbibed.

The litharge runs off in the ſmall proceſs;

This is not the caſe in large cupels of ſeveral yards diameter. Bellows muſt be uſed, not only to accelerate the oxidation by their blaſt, but to drive the litharge towards the paſſage or gutter which is left for its iſſue.

—but cannot in the large.

The inconveniencies and even the impoſſibility of cauſing all the lead to penetrate the aſhes of theſe large cupels have been already remarked. This muſt be evident on reflecting that the oxidation can take place only at thoſe parts of the bath which are expoſed to the contact of the air and the blaſt of the bellows. The litharge near the middle of the baſon not being diſpoſed to flow towards the edge, would cover and defend the metal from any farther oxidation. Hence it is that the operators have found themſelves obliged to drive out the litharge by the mechanical action of a ſtream of air from bellows.

It muſt be driven off by bellows.

The oxidation therefore takes place only at the ſurface of the lead, and not lower; if it were otherwiſe, the aſhes of the cupel would be penetrated with oxide to a depth which would be more unequal the longer the operation laſted. Now I have always remarked that the portion of aſhes thus imbibed in the large refinery is not thicker towards the center of the baſon than towards its edge, though the lead remains thirty or forty times longer at the bottom than near the edges, becauſe the bath

The oxidation is effected only at the ſurface.

bath constantly diminishes till all the lead is reduced into litharge, and nothing at last remains but the button of silver at the bottom of the cupel.

Other reasons why the large process cannot be made by absorption.

That the whole of the lead is absorbed in the cupel of assay, arises from the unequal action of heat upon every part of the small vessel. As the cupel in the large way presents only its superior surface to the action of the heat, the oxide thus imbibed ceases to penetrate at the place where the temperature is no longer in a state to hold the oxiding fusion. For this reason it is that the whole of the cupel is throughout impregnated to an equal depth, and it is impossible to cause all the lead to penetrate the ashes.

Litharge is sought as a product in the large way.

From the preceding observations it will be easy to conclude that though the assay of lead must be made in small cupels of bone ashes, in order that the whole of the oxided metal may either be absorbed or partly evaporated; yet the case is very different in the large operation, where the object is to proceed with celerity, and to obtain as much litharge as possible.

Addition of sand to the wood ashes, &c.

I have before stated that the wood ashes used in forming large cupels are expensive, and frequently not to be procured in sufficient quantity; to which I have added their being subject to blow up or rise entirely, which occasions a considerable loss. It must further be mentioned, that in order to give more weight and consistence to these cupels it is often necessary to mix a considerable quantity of sand with them, particularly if the lead should contain foreign substances, such as arsenic, cobalt, antimony, zinc, tin and other matters. If the lead be merely arsenical, after having taken off the first scum, it is usual to throw from time to time, on the whole surface of the bath, about 20lbs. of iron scales or granulated crude iron. This iron being lighter than the lead, floats on the top and absorbs the arsenic, after which it is cleared away, and then the litharge is formed without any obstacle. This method is used in Saxony.

The necessity of adding sand to the ashes of the cupels ought to have led to a discovery which I propose: it is as follows.

New Construction of the Cupels or Basons for refining Lead.

New construction of the basons or vessels for refining lead.

Without making any change in the masonry of the furnaces for refining by what we call the German method, it is only necessary to be careful to make a sufficient number of vents in their

their base for the evaporation of the moisture, and to dispose of them so as most effectually to answer this purpose. These channels or vents are to be covered with a bed of scoria, upon which a pavement is to be made of the most porous bricks, and of the thickness of a single brick.

On this area or pavement, which ought to be concave like the base upon which the ashes of the ordinary cupels are placed, must be laid a quantity of founder's sand, a little moistened. If it be not adhesive enough, a little clay may be added, in order to give the requisite solidity, and the whole carefully mixed. The sand must be rammed down in the same manner as is done to consolidate the ashes in the usual way, and a bason for refining must be formed, equally rammed in all its parts. The thickness of this cupel should be about six inches; and it may be made in two layers, as we shall hereafter observe.

The cupel is made of sand with a little clay instead of bone ashes.

After the bason has been in all parts uniformly beaten or rammed down, it will be proper to sift over the whole surface two or three quarts of wood ashes, which may be made to adhere by ramming.

When the cupel is thus prepared, the head of the furnace must be lowered, and a moderate fire kept up for several hours, in order to evaporate part of the water from the sand. The rest will be driven out, without inconvenience, through the vents during the refining.

It may be dried before use;

After a sufficient drying, which may even be dispensed with; the head is to be raised, the cupel, suffered to cool a little, and straw or hay then laid upon it, and upon this the pigs or pieces of lead, which are to be gently put down, in order that their weight may not make impressions in the sand. The straw is used for this purpose, in our method as well as the common method; and it would be convenient that the lead should be cast in iron hemispherical moulds or pots instead of the prismatic form, as these pieces would be less subject to damage the cupel.

—but this is not absolutely necessary.

Method of charging with lead.

When the quantity of lead necessary to fill the cupel is arranged in the furnace, the head is to be lowered and luted all round with clay, after which the fire is to be applied as in the usual processes.

When the lead is in perfect fusion, and the bath covered with dross and coally matter from the straw, this skum must be

Fusion.

Clearing of the surface.

 raked

raked off through the paſſage left for the litharge, by means of a wooden rake about a foot long, with an iron handle of ſufficient length to reach every part of the bath.

Method of blowing, &c.

When the lead has been ſeveral times ſkimmed, and begins to become red, the bellows muſt be ſet into action, gently at firſt, and afterwards more ſtrongly. Their nozles muſt be ſo diſpoſed that the blaſt may be directed towards the center of the bath, and in order that the wind may be urged upon the ſurface of the metal, each nozle muſt have a ſmall round plate of iron adapted to it. Theſe ſmall flaps or valves, called papillons (flies or butterflies) are uſed in the German fineries. They have an hinge at top, and at every ſtroke they riſe about half way from their perpendicular poſition towards the level, ſo that by reflecting the air downwards upon the lead, they haſten its oxidation.

Gutter for the litharge.

When all the droſs has been removed, and the lead is of a good red heat and covered with litharge, a little gutter muſt be made, with an hook appropriated for this purpoſe, in the ſand of the cupel. This muſt be carefully done until the bottom of the gutter anſwers to the level of the bath. The litharge driven by the blaſt of the bellows will flow out of this paſſage, and fall upon the hearth of the foundery.

Inſtructions.

When the operator perceives that only a ſmall quantity of litharge remains near the gutter, he will ſtop its eſcape with a ſmall quantity of moiſtened aſhes; but as ſoon as the lead ſhall be again covered with oxide, the paſſage muſt be opened and made deeper as the quantity of matter becomes depreſſed; taking care that no lead eſcapes, particularly towards the end of the operation, as it would carry along with it a large portion of ſilver which would be loſt.

In this manner the proceſs is to be carried on until the ſurface of the ſilver exhibits thoſe flaſhes which are called the brightening, taking care to raiſe the fire in proportion to the diminution of the bath, particularly towards the end, when the ſilver is collected; and as this metal is much more difficult to be kept in fuſion than its ſmall alloy of lead, the refining will be imperfect, unleſs the temperature be raiſed, and inſtead of about one twentieth of lead, which the ſilver uſually retains in the German method, it will remain much more highly charged. This would render it more difficult to be treated in the ſecond operation, called the ſilver refining, or by the Germans *ſilver brenen*, by which it is rendered pure.

5 Thoſe

Those who are accustomed to refine lead in the German method, will find no difficulty in following mine. For though the cupel is made of sand instead of ashes, there is no difference in the manipulations.

We have seen that the English refine a great quantity of lead on a small cupel. The same may be done in the method here described, by adding in proportion as the loss by oxidation takes place. Supposing the capacity of the cupel to be such as to contain about five ton of lead, we might continue the operation to three times that quantity in a single process, which would not have the inconveniences of the English method. This method admits of keeping up the charge during work,

I flatter myself that a well-made cupel of sand may be used for several refinings, without requiring to be made up again every time like those of ashes; but in these circumstances, and before the lead is put in, it is necessary to fill with well beaten sand the gutter or opening which was before made for carrying off the litharge. Care must be taken in doing this to remove with a chissel that kind of glaze which the oxide of the lead leaves behind it. With this precaution, after wetting the part where the new moistened sand is to be applied, they will firmly unite together. and the cupel may be repeatedly used.

From the long duration of the earthen floors of those reverberatory furnaces in which lead ores and even litharge are fused, as was before mentioned, we have no reason to apprehend any bad consequences from the oxide of lead, which acts only on the surface of the cupel, and penetrates to an inconsiderable depth.

After one or two refinings, this crust of oxide may be taken off and fused in a blast furnace, in order to recover the lead; a process no less easy than that of reducing the metal which exists in much greater quantity in the ordinary cupels. We therefore obtain a larger quantity of litharge, which is one advantage, and in addition to this, the loss in silver which accompanies the absorbed lead will be less. For in the small quantity of precious metal which accompanies the oxide, it is found by experiment, that the proportion is greater in the absorbed lead than in that which is driven over in litharge. Recovery of the absorbed lead.

Instead of sand we might make use of clay in constructing our cupels, as is done in the hearths of the reverberatory furnaces of Brittany; but it would then be necessary to pound the To make the cupel of clay instead of sand would be more expensive and less convenient.

the earth repeatedly for ſeveral days, otherwiſe it would crack, and theſe cracks, which would become wider by the ſhrinking from heat, would afford a lodgment for ſome of the lead; an inconvenience which ſand, even if rather loamy, does not preſent. It muſt alſo be remarked, that a cupel of clay would become too hard to admit of the excavation for carrying off the litharge; ſo that this part at leaſt would require to be made of ſand or aſhes.

Two kinds of ſand.

It will be advantageous to uſe two kinds of ſand in forming a baſon of the cupel, the one fine ſuch as the founders' ſand, and the other coarſe. The latter may form the firſt ſtratum, which, after being well rammed with the implements uſed for this purpoſe, muſt be left about three inches in thickneſs. Upon this the fine and ſomewhat loamy ſand is to be ſpread and rammed like the firſt. A ſlight degree of moiſture muſt be uſed with both theſe, in order that they may more ſolidly adhere together. The lower ſtratum being more coarſe, will facilitate the eſcape of the humidity,

Repair or renewal of the cupel.

It will not be neceſſary to diſturb the lower ſtratum of ſand when a new cupel is to be made; and even of this laſt that portion which has not imbibed any oxide may be uſed along with the new ſand intended to be applied. The lower ſtratum muſt not be touched during this renewal, for fear of mixing coarſe ſand with the fine. This inconvenience may be guarded againſt by ramming upon the ſurface of the coarſe ſand a bed of a thin facing of aſhes, at which the operator muſt ſtop when he takes away the upper ſtratum.

We have remarked that the founders' ſand muſt be rather loamy, and that if it be not ſo, it will be neceſſary to add a ſmall quantity of clay to render it adheſive; but as it is neceſſary that this clay ſhould be equally diffuſed through the maſs, it may be diffuſed in the water with which the ſand is to be ſprinkled, and the whole muſt be carefully mixed.

Abſorption is of no advantage in the old proceſs.

It might be objected, that ſince the cupels of ſand do not abſorb ſo much litharge as thoſe of aſhes, more time will be required to complete the refining, becauſe the oxide inſtead of being in part abſorbed, muſt by this new proceſs be driven out of the furnace. This however is a ſubject which ought not to be conſidered as of any importance; for the blaſt of the bellows well directed will cauſe the oxide of litharge to flow out more abundantly through the gutter than if the abſorption took place.

I have

I have seen operators in Germany, who, when they constructed their cupels, had the precaution to form a small circular cavity in the middle, the depth of which was proportioned to the quantity of silver, which from the assay they knew to be contained in the lead of one operation. By this contrivance there were no insulated grains of the metal left on the surface of their cupel, but the whole of the silver formed a perfectly round cake in the middle of the center excavation. I would advise the same ingenious expedient to be used in the cupels of sand.

Improvement. Cavity to receive the silver.

I am well assured that the cupels I propose, if made with care and attention, will succeed perfectly, and that, independent of their convenience beyond the others, they will be found very economical. I am desirous that, for the advantage of metallurgy, this method should be generally used, and its benefits will prove that we ought not always to follow with servility the established usages nor the common working processes.

Conclusion.

XI.

Letter from Mr. James Stodart, *explaining the Method of gilding upon Steel by Immersion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture.*

To Mr. NICHOLSON.

Dear Sir,

A Considerable degree of public interest and curiosity has lately been excited by the exhibition of instruments of steel coated or gilt with gold. The discovery, although not altogether new, does not appear to be very generally known; and as its application to various manufactures promises to be both elegant and useful, I have, with a view of saving some time and expence to others who may be inclined to make further experiments, added a short account of a method which with me has succeeded perfectly well. I wish here in justice to observe, that this discovery of the method belongs more to my friend Mr. Hume, chemist, Long-Acre, than to myself. With that gentleman's kind assistance I had but few difficulties to

Account of the process. Three parts sulphuric ether are added to one of solution of gold. The gold is taken up by the ether.

to overcome. The following is our method: To a saturated solution of gold in nitro-muriatic acid, add about three times the quantity of pure sulphuric ether: Agitate them together for a short time. The gold will soon be taken up by the ether in the form of a muriate, or nitro-muriate of gold, leaving the remaining acid colourless at the bottom of the vessel, which must now be drawn off by means of a stop-cock, or other similar contrivance. The acid being discharged, the instrument to be gilt having been previously well polished and wiped very clean, is to be dipped for an instant into the ethereal solution, and on withdrawing it, as instantly washed by agitation in clean water. This is essential to get clear of a small portion of acid necessarily taken up with the metal; and if this be neatly done, the surface of the steel will be completely and very beautifully covered with gold. Some little degree of dexterity is required to perform the whole operation well.

The instrument to be dipped into the ethereal solution.

Essential oils do not succeed well.

I have tried some of the essential oils, knowing that they will take the gold from nitro-muriatic acid; but as far as I went they did not apply for the purpose of gilding: and as I had found all I wanted in ether, I certainly did not prosecute the other experiments with much industry.

I remain, with much respect,

Dear Sir,

Your obliged servant,

J. STODART.

Strand, June 24, 1805.

XII.

On the peculiar Noise emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N.

Introduction.

SOME time ago a philosophical friend who favoured me with a visit, mentioned in conversation that the simmering of water before it boils had formed the subject of enquiry between himself and other curious examiners of natural appearances, as being a fact not yet well explained. It seemed to me very remarkable, as I dare say it will to my readers, that any of

of the facts relating to the boiling of water, concerning which so much has been said and written, should still remain in obscurity. Having, myself, been in the habit of considering it as the consequence of a rapid escape of interspersed air from the heated water, I requested he would say why he thought the phenomenon repugnant to that supposition. My friend proceeded in reply to state, that when water is first put on the fire, in a metallic vessel, the vessel itself gradually becomes lined with bubbles; that these bubbles become detached and rise, so that the whole surface, or at least the bottom, becomes clear again; that, soon afterwards a rattling noise is heard, sharp and metallic, which encreases in loudness until it almost perfectly resembles the pouring of small shot into the vessel; that the fluid continues in a state of tranquillity and transparency during this state; and lastly, that when the noise is loudest the state of ebullition suddenly comes on, the peculiar noise of simmering ceases all at once, and nothing is heard but the soft and moderate noise of aquatic agitation as long as the boiling lasts.

Particular description of the act of simmering.

This account, so remarkable for its precision and accuracy, shewed clearly that my notion, which I believe is the common opinion, was ill founded. After a little meditation, it appeared evident to me that the noise of simmering must arise from the collapsion of steam bubbles, formed at the bottom of the vessel, and condensed almost instantly upon their ascent in the fluid not yet heated to the boiling point. In support of this opinion I shewed him a common experiment with the water-hammer. This instrument, which is made and sold by the glass-blowers and barometer-makers, consists of a tube, nearly a foot in length and about three quarters of an inch in diameter, terminating in a globe of about two inches in diameter; the other end of the tube being closed. The outer extremity of the globe ends in a capillary tube, through which as much water has been introduced as rather more than fills the globe itself. This water has been boiled in the vessel or instrument, and at the time of boiling, when all the internal cavity not containing water was filled with steam to the almost total exclusion of air, the capillary aperture was hermetically closed. The instrument thus completed is found when cold to contain water and a space nearly vacuous, and the experiment from which it derives its name of

It seems to arise from the sudden production and condensation of steam.

Description of the water-hammer.

Singular noise of water agitated in vacuo.

of the water-hammer, is that of agitating its contents. The remarkable effect it exhibits is, that the parts of the water strike against the glass and against each other, with the sharp noise usually produced by the collision of hard bodies. Another experiment, which I alluded to, is that, if the ball be held downwards so as to become filled with the water, and it be then slowly raised up, so as to bring the tube nearer to the horizontal position, the heat of the hand which holds the tube will produce vapour or steam strong enough to prevent the fluid from running down to its level from the upper surface of the globe; but a little farther inclination causes it to descend, and a bubble of the steam enters the globe itself and ascends through the fluid. But it scarcely arrives at the top before it is condensed, and the water collapses with a smart stroke or noise, so as to fill the globe again. The succession of these bubbles and their condensation take place so rapidly, in a well made instrument, that ten or twelve collapsions occur in every second of time. The fact, and the obvious remarks I made upon it, convinced my friend that I had suggested the proper explanation.

Another experiment of steam produced and condensed twelve times in a second,

applied to explain the effect of simmering.

Objections.

It was my intention however to have heated some water in a vessel in order to observe and ascertain the progressive appearances, but I had not done it when I again had the pleasure to meet this intelligent observer. He complimented me upon the ingenuity of my solution, but having himself since repeated the experiment of boiling water, it seemed from his report that ingenuity was all the value it could claim. "Take a bright tin vessel" said he "and heat water in it; you will hear the noise, but no bubbles are to be seen."

I took the earliest opportunity of making some experiments, the particulars and results of which are as follow:

Exp. 1. Water was heated in a glass retort. The simmering was attended with steam bubbles.

Exp. 1. A small glass retort, the body of which is about two inches in its shortest or horizontal diameter, was suspended so that its neck was elevated about twenty degrees above the level. Water was then poured in to fill the body and the greatest part of the neck. My intention in filling the neck was, that I might be able to observe whether any greater or more sudden rise took place before the period of boiling than the well known expansion of the fluid. A small spirit-lamp was placed beneath the bulb. The coldness of the water in the vessel immediately condensed a portion of the water which issued

issued from the flame itself, and formed drops on the outside. As the included water became heated this condensed water evaporated, and left the surface again clear; and at this period the disseminated air began to separate and gave a dusty appearance to the inner surface, which lasted about three minutes. At the end of this time the inner surface began to clear; the peculiar noise of simmering was heard; and bubbles were seen suddenly appearing and collapsing; the retort itself being agitated and the surface of the water rising and falling by starts. The bubbles were pointed at top, somewhat resembling small flames suddenly appearing and vanishing at different parts of the surface. In the course of one minute they grew larger and larger and collapsed at greater heights, until at length they escaped through the fluid without being condensed. This was the instant of ebullition or boiling and at this period the noise of simmering ceased and that of boiling was heard.

Exp. 2. As this effect appears to arise decidedly from the upper water being colder than that near the bottom of the vessel, it was natural to infer that the appearances would be different according to its figure and magnitude. I therefore took a bolt-head, or spherical glass body, with a straight neck: Its diameter was four inches; and when it was filled, a column of water eight inches long stood in its upright neck. The thickness of the glass at its bottom was considerable. At 35 minutes after three the lamp was lighted. At 40 minutes bubbles of gas rose singly, and very little of the dusty appearance was seen. At 58 minutes the noise of simmering began, and the collapsing bubbles were plentiful and distinct. Little streams or fountains of steam rose from particular points and were condensed; and some globes of half an inch diameter ascended clear of the bottom and collapsed in the fluid above. At one minute after four the bubbles reached the top of the fluid without collapsing, and at this time the noise of simmering ceased. The lamp was then blown out.

Exp. 2. Water was heated in a larger glass vessel. The effects were rather more evident.

Exp. 4. A bright copper hemisphere, four inches in diameter, was filled with water at the temperature of 60 degrees. At four hours three minutes the lamp was lighted and water became condensed on the outside. At five minutes the inside surface had a dusty appearance, from bubbles of air immediately over the flame. The temperature was then 110°, and the

Exp. 4. The experiment was repeated in a copper vessel.

the outside dry. At six minutes bubbles of gas or air were detached and rose, the temperature being 126°, and the vapour of steam being visible from the surface of the water. At eight minutes the inside surface was coated with large bubbles or beads of air; temperature 150°. At nine minutes, temp. 165°, much vapour. At 9½ min. temp. 175°, the bottom was clear of bubbles, and the noise of simmering began. At 10 min. temp. 184°, the steam collapsing bubbles were very evident, though, from their pointed shape, they were not immediately obvious to an observer looking straight down into the shining vessel. At 11 min. temp. 180°, noise very loud and bubbles more and larger. At 12 min. temp. 185°, some of the bubbles broke at the surface, and the noise was less. At 12¾ min. temp. 204°, the water boiled, and the simmering noise was succeeded by that of boiling. At 14 min. the lamp was extinguished.

Exp. 5. Water already heated does not simmer so much or so loudly as water quickly raised from a low temperature.

Exp. 5. The water was suffered to cool to 170°, and the lamp was then lighted again, namely, at four hours 18 minutes. At 20 min. temperature 180°, the simmering began; but not till after the steam bubbles were seen very large: and soon afterwards, at 204°, they rose through the fluid, and the boiling took place by fountains or streams of bubbles rising from particular points. The simmering noise in this experiment was much less than before.

The thermometer was placed horizontally, with the greatest part of its tube, and part of its bulb, out of the water. When the bulb was plunged in the water, it shewed 208°.

Conclusion.

From these facts it appears to be clearly established, that the cause first mentioned, namely, the condensation of steam bubbles in their ascent through the cold fluid above, is the occasion of the noise of simmering. In the fifth experiment the superincumbent water was hotter than that beneath, and consequently the simmering could not be produced but by steam at a higher temperature, and even then the collapsion of the water was less sudden and the noise less loud.

Experiments

XIII.

Experiments on Wootz. By Mr. DAVID MUSHET. *From the Philosophical Transactions,* 1805.

THE following experiments were made at the requeſt of Sir Joſeph Banks, on five cakes of wootz, with which he ſupplied me for that purpoſe. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the ſame quality, it will be proper firſt to deſcribe the differences obſervable in their external form and appearance. **Experiments on five cakes of wootz.**

No. 1 was a denſe ſolid cake, without any flaw or fungous appearance upon the flat, or, what I ſuppoſe to be, the upper ſide. The round or under ſurface was covered with ſmall pits or hollows, two of which were of conſiderable depth; one through which the ſlit or cut had run, and another nearly as large towards the edge of the cake. Theſe depreſſions, the effects, as I ſuppoſe, of a ſpecies of cryſtallization in cooling, were continued round the edges, and even approached a little way upon the upper ſurface of the wootz. **Deſcription of the cake, No. 1.**

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical poſition. Its convexity, compared to that of the other five, was ſecond. Upon breaking the thin fin of ſteel, which connects the half cakes together, I found it to poſſeſs a very ſmall denſe white grain. This appearance never takes place but with ſteel of the beſt quality, and is leſs frequent in very high ſteel, though the quality be otherwiſe good. **Fractured very ſteely.**

Upon examining the break with attention, I perceived ſeveral laminæ and minute cells filled with ruſt, which in working are never expected to unite or ſhut together. The grain otherwiſe was uniformly regular in point of colour and ſize, and poſſeſſed a favourable appearance of ſteel.

No. 2. This cake had two very different aſpects; one ſide was denſe and regular, the other hollow, ſpongy, and protuberant. The under ſurface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one ſide, it became flatter. The grain expoſed by breaking was larger, bluer in colour, and **Deſcription of the cake of wootz, No. 2.**

and more ſparkling than No. 1. In breaking, the fracture tore but ſlightly out, and diſplayed the ſame unconnected laminæ with ruſty ſurfaces, as was obſerved in No. 1. Beſide theſe, two thin fins of malleable iron projected from the unſound ſide, and ſeemed incorporated with the maſs of ſteel throughout. Towards the centre of the break, and near to the excreſcence common to all the cakes, groups of malleable grains were diſtinctly viſible. The ſame appearance, though in a ſlighter degree, manifeſted itſelf in various places throughout the break.

Cake of wootz, No. 3.

No. 3. The upper ſurface of this cake contained ſeveral deep pits, which ſeemed to reſult from the want of proper fluidity in fuſion. They differed materially from thoſe deſcribed upon the convex ſides of No. 1 and 2, and were of that kind that would materially effect the ſteel in forging.

The under or convex ſide of this cake preſented a few cryſtalline depreſſions, and thoſe very ſmall; the convexity was greater than that of No. 1 and 2, the fracture of the fin almoſt ſmooth, and only in one place exhibited a ſmall degree of tenacity in the act of parting. In the middle of the break, about half an inch of ſoft ſteel was evident; and in different ſpots throughout numerous groups of malleable grains, and thin laminæ of ſoft blue tough iron made their appearance.

Cake of wootz, No. 4.

No. 4. Was a thick denſe cake poſſeſſed of the greateſt convexity; the depreſſions upon the under ſide were neither ſo large, nor ſo numerous as thoſe in No. 1 and 2, nor did they approach the upper ſurface of the cake further than the acute edge. This ſurface had the moſt evident marks of hammering to depreſs the feeder, or fungous part of the metal, which in the manufacturing ſeems the gate or orifice by which the metal deſcends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being ſolid. Towards the feeder it ſeemed looſe and crumbly, and much oxidated. The grain divided itſelf into two diſtinct ſtrata, one of a denſe whitiſh colour, the other large and bluiſh, containing a number of ſmall ſpecks of great brilliancy. Several irregular lines of malleable iron pervaded the maſs in various places, which indicated a compound too heterogeneous for good ſteel.

Cake of wootz, No. 5.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet was

was smooth and free from depressions, or honey-comb on both surfaces. The feeder, instead of being an excrescence, presented a deep concave beautifully crystallized.

In breaking, the fracture tore out considerably, but presented a very irregular quality of grain. That towards the under surface was small and uniform, but towards the flat or upper surface it increased in size, and in the blueness of its colour, till it passed into the state of malleable iron.

The break of this steel, though apparently soft, was the least homogeneous of the whole, and throughout it presented a very brilliant arrangement of crystal, which in other steel is always viewed with suspicion.

General Remark.

Uniformly the grain and density of the wootz are homogeneous, and free from malleable iron towards the under or round surface; but always the reverse towards the feeder or upper side.

Remarks in Forging.

No. 1. One-half of the cake was heated slowly by an annealing heat to a deep red, and put under a sharp broad-mouthed chissel with a small degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the slit or cut originally in the cake. The appearances on forging the cake of wootz, No. 1.

The heat in this trial was so moderate, that I was afraid that the crack had arisen from a want of tenacity, occasioned by the heat being too low.

The other half was heated a few shades higher, and subjected to the same mode of cutting; before the chissel had half way reached the bottom, the piece parted in two in the direction of the depression made by the cutting instrument. The additional heat in this instance proved an injury, while the cracking of the steel in both cases, particularly the former, was a certain proof of the abundance, or rather of the excess of the steely principle.

The fractures of both half cakes, now obtained for a second time, were materially different from that obtained by the simple division of the cake. The grain was nearly uniform, distinctly marked, but of too gray a colour for serviceable steel. Two of the quarters being drawn into neat bars under hand hammers

The appearances on forging the cake of wootz, No. 1.

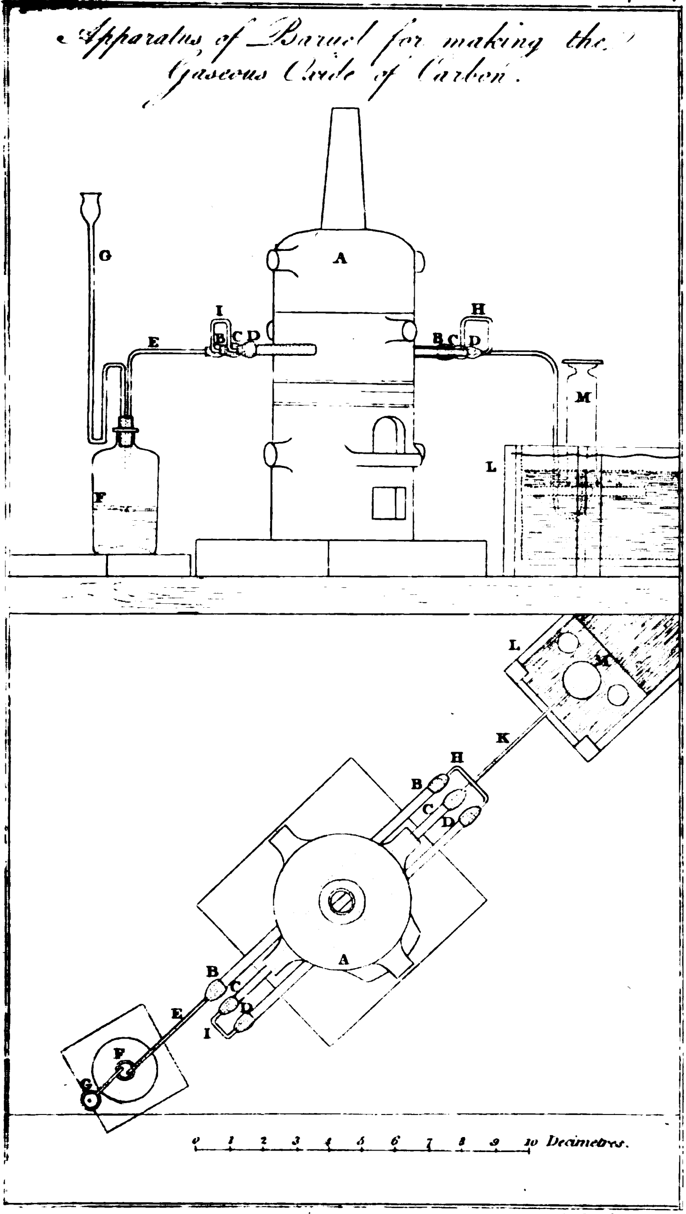
mers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolky and excessively dense. A small bar of penknife size was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break, than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

(To be continued.)

*** *I have received a letter from Mr.* BOSWELL, *in which he expresses an opinion, that it is unfair in the Old Correspondent whose letter appeared in our last Number to have applied Mr. B's apology to one particular part of his paper; as he conceives, that it ought to have been considered as indicating the spirit in which his whole communication was written. I have inserted this short notice out of respect to the writer; but have declined inserting the letter itself, because the controversy can have no farther importance to the readers of this Journal, after the subject itself has been exhausted.*

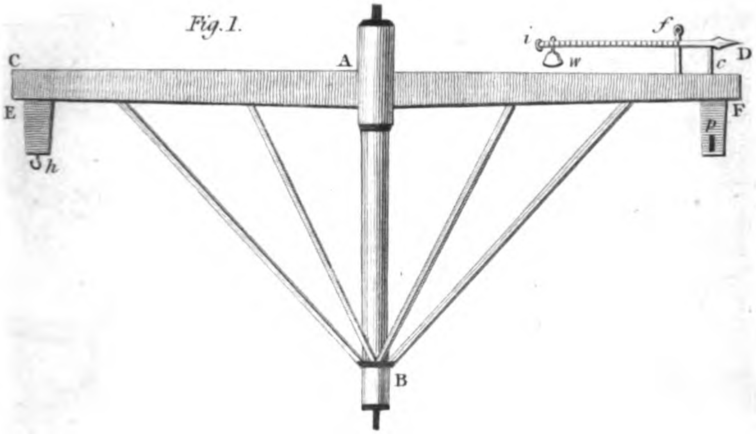


Mutlow sc. Russell Co.


Mr. Churchman's
Topographical Plan
Horizontal Distances
Scale of Perches.
CANAL
CANAL
Elevation
Elevation
Depth
Obelisk
Perpendicular Heights
Perches

Mr. Gregory's Contrivance to determine Horse-power in Mills.

Fig. 1.

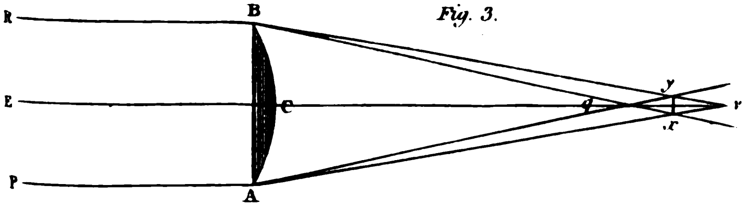


American Borer.

Fig. 2.

Mr. Ez. Walker on Light.

Fig. 3.



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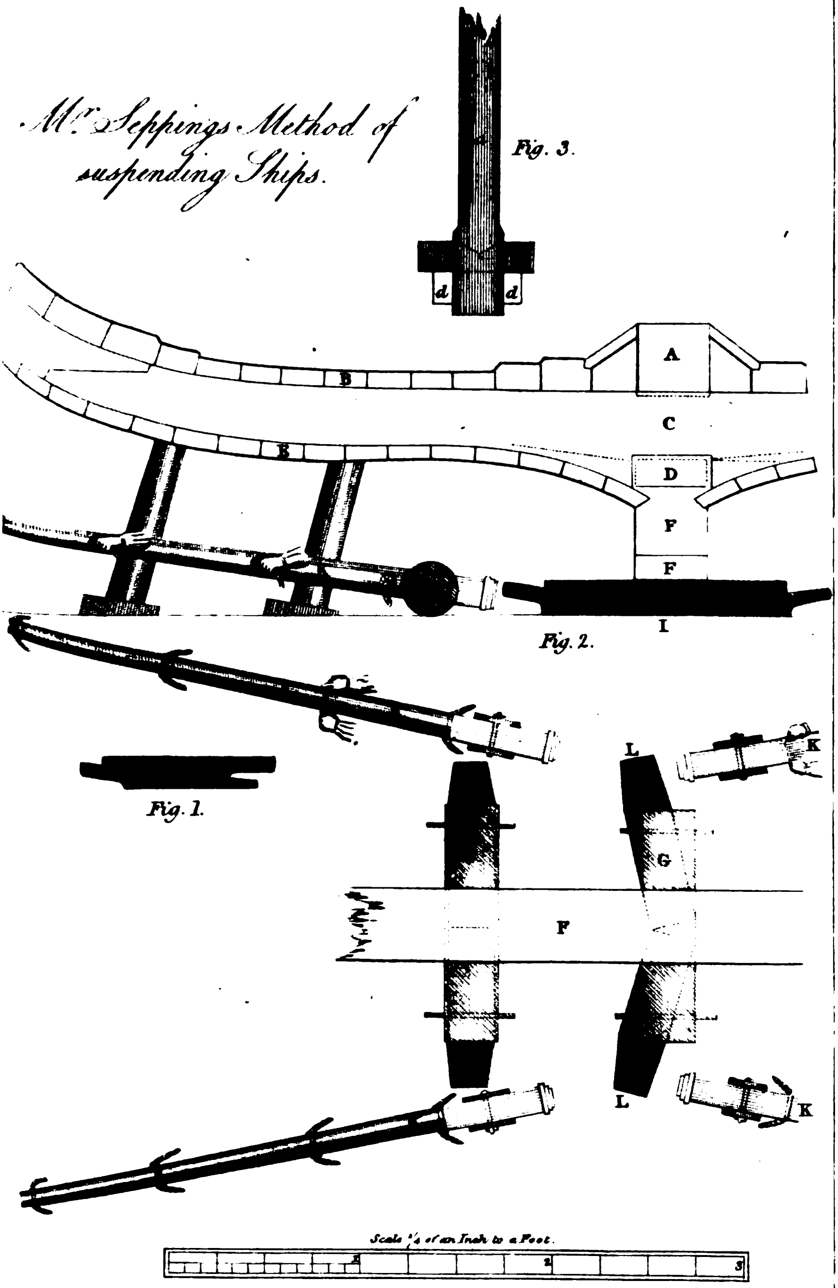


Mallow Sc. Rupert St.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1805.

ARTICLE I.

An Account of some new Experiments which prove that the Temperature at which the Density of Water is a Maximum, is several Degrees of the Thermometer above the freezing Point. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c. Received July* 16, 1805, *from the Author; with a Letter dated Munich*, 25*th June*, 1805.

IN my seventh essay, in which I have treated of the propagation of heat in fluids, and also in a paper published in the Philosophical Transactions for the Year 1804, Part I.; in which I have given an account of a curious phenomenon frequently observed on the Glaciers of Chamouny, I have ascribed the melting of ice which is placed (by design, or by accident) below the surface of ice-cold water, to currents of warmer water, which, in certain cases, are supposed to descend in that ice-cold liquid: but as this supposed fact has lately been called in question by several persons, and as the explanations I have founded on it must fall to the ground, unless it can be supported, I have been induced to re-consider the matter, and to give it a careful and thorough investigation.

Phenomena which have been explained from the maximum density of water being at higher than freezing temperature:

the fact questioned.

It was first announced by De Luc.

The fundamental fact on which the supposition is grounded, which was announced many years ago by Mr. De Luc; namely, that the temperature at which the density of water is a maximum, is considerably higher than that at which that fluid freezes, is indeed so very extraordinary, and appears to be the cause of so many interesting phenomena, that too much pains cannot be taken to put it beyond doubt.

New method of proving it.

As the methods hitherto used for determining that important point have, by some at least, been considered as insufficient, I shall take the liberty to propose another, by which the fact in question may, I think, be demonstrated directly; and without any nice calculations, or any very difficult or delicate experiments.

Let the following experiments, (which it will be easy to repeat) speak for themselves.

Apparatus. In the middle of a thin cylindrical brass vessel, a thin brass cup is supported.

Having provided a cylindrical vessel, (A Fig. 1. Plate XII.) open above, made of thin sheet brass, which is 5½ inches in diameter, and four inches deep, supported on three strong legs, 1¾ inches high; I placed in it a thin brass cup, (B) two inches in diameter at its bottom, (which is a little convex downwards) 2 $\frac{8}{10}$ inches wide at its brim; and 1 $\frac{3}{10}$ inches deep; which cup stands on three spreading legs made of strong brass wire, and of such form and length, that when the cup is introduced into the cylindrical vessel, it remains firmly fixed in the axis of it, and in such a situation, that the bottom of the cup is elevated just 1¾ inches above the bottom of the cylindrical vessel.

In the middle of the brass cup is supported

In the middle of this cup there stands a vertical tube of thin sheet brass, ½ an inch in diameter, and $\frac{6}{10}$ of an inch in length, open above, which serves as a support for another smaller cup (C) which is made of cork; the brim of which is on the same horizontal level with the brim of the larger brass cup, in which it is placed.

an hemispherical cup of cork,

This cork cup, which is spherical, (being something less than half of an hollow sphere) is one inch in diameter at its brim, measured within $\frac{4}{10}$ of an inch deep, and ¼ of an inch in thickness. It is firmly attached to the vertical tube on which it stands, by means of a cylindrical foot, ½ an inch in diameter, and ¼ of an inch high; which, when some force is employed, enters into the opening of the vertical tube.

On

On one side of this cork cup there is a small opening, which receives, and in which is confined the lower extremity of the tube of a small mercurial thermometer, (D). The bulb of this thermometer, which is spherical, is just $\frac{3}{10}$ of an inch in diameter, and it is so fixed in the middle of the cup, that its centre is just $\frac{1}{4}$ of an inch above the bottom of the cup; consequently it does not touch the cup any where, nor does any part of it project above the level of its brim. having in its center the bulb of

The tube of this thermometer, which is six inches in length, has an elbow near its lower end, at the distance of one inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is so fixed in the cup, that the short branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal position, while the longer branch (to which a scale, made of ivory, and graduated according to Fahrenheit is affixed,) projects obliquely upwards and outwards, in such a manner that the freezing point of the scale lies just above the level of the top of the cylindrical vessel in which the cups are placed. a thermometer.

The cork cup, which was turned in the lathe, is neatly formed, and in order to close the pores of the cork, it was covered, within and without, with a thin coating of melted wax; which was polished after the wax was cold. Other particulars.

The thermometer was fixed to the cork cup by means of wax, and in doing this, care was taken to preserve the regular form of the cup, both within and without.

The vertical brass tube which supports this cup in the axis of the brass cup, is pierced with many holes, in order to allow a free passage into it, and through its sides, to the water employed in the experiments.

Having attached about six ounces of lead to each of the legs of the brass cup, in order to render it the more steady in its place, it was now introduced with its contents, into the cylindrical vessel, and the vessel was placed in an earthen basin (E) seven inches in diameter below, 11 inches in diameter at its brim, and five inches deep, and was surrounded on all sides with pounded ice. This apparatus was placed in a pan, and surrounded with ice.

Several flat cakes of solid ice were now put into the cylindrical vessel, and fastened down upon its bottom, and under the bottom of the brass cup, and a circular row of other long pieces of ice were placed, in a vertical position, round the outside Ice was placed and secured in the cylindrical vessel; but not in the cups, and ice-cold water was poured

into all the remaining space. outside of the brim of the brass cup, between it and the vertical side of the cylindrical vessel, which vertical pieces of ice reached upwards to within about $\frac{1}{10}$ of an inch of the top or brim of the cylindrical vessel; and when this was done, ice-cold water was poured into this vessel till the surface of that cold fluid stood just one inch above the level of the brim of the cork cup.—Both cups were of course submerged and filled with ice-cold water, and surrounded on all sides by solid cakes of ice.

The cups remained thus submerged for one hour. After things had remained in this situation more than an hour, during which time the cold water in the cylindrical vessel, and that in the cups, was frequently agitated with the soft end of a strong feather, and the cups and the water in every part of the vessel appearing to be exactly at the temperature of freezing: I proceeded to make the following decisive experiment.

Experiment. No. 1.

Experiment I. A cone of metal at 42° was plunged in the ice-cold water, just above the cork cup: A solid ball (F) of tin having been provided, two inches in diameter, with a cylindrical projection on the lower side of it, one inch in diameter, and half an inch long, ending in a conical point, which projected (downwards) half an inch farther; this ball (to which was fixed a strong iron wire six inches in length, which served as a handle to it,) having been made to acquire the temperature of 42° F. by keeping it immersed near half an hour in a large quantity of water at that temperature, was placed as expeditiously as possible over the middle of the cork cup, and in such a situation that the whole of the descending conical point (half an inch in length) was immersed in the ice-cold water in the cylindrical vessel, the extremity of that point being just half an inch perpendicular above the upper side of the bulb of the thermometer which lay in the cork cup.

It was foreseen, that if the water in contact with the cone became denser by the heat, it would descend and raise the thermometer. I knew that the particles of ice-cold water which were thus brought into contact with the conical point, could not fail to acquire some small degree of heat from that relatively warm metal; and I concluded, that if the particles of water so warmed should in fact become *heavier* than they were before, in consequence of this small increase of temperature, they must necessarily *descend* in the surrounding lighter ice-cold liquid, and as the heated metallic point was placed directly

directly over the cork cup, and fixed immoveably in that situation, I foresaw that the descending current of warm water must necessarily fall into that cup, and at length fill it, and that the presence of this warm water in the cup would be announced by the rising of the thermometer.

The result of this very interesting experiment was just what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 seconds, when the mercury in the thermometer began to rise, and in three minutes it had risen three degrees and a half, namely, from 32° to 35½°, when five minutes had elapsed, it had risen to 36°, when an end was put to the experiment.

It did, in fact, raise the thermometer in the cup;

Another small thermometer, placed just below the surface of the ice-cold water, and only $\frac{2}{10}$ of an inch from the upper part of the conical point, on one side of it, did not appear to be sensibly affected by the vicinity of that warm body.

but it did not affect a thermometer near the surface of the water;

A third thermometer, the bulb of which was placed in the brass cup, just on the outside of the cork cup, and on a level with its brim, shewed that the water which immediately surrounded the cork cup, remained constantly at the temperature of freezing, during the whole time that the experiment lasted.

nor another near the outside of the cup.

As I well knew, from the results of the experiments on the propagation of heat in a solid bar of metal, of which an account has been given in a memoir presented to the first class of the National Institute of France, on the 7th of May 1804, that the ice-cold water in this experiment could not possibly acquire from a contact with the conical metallic point, a temperature so high as that of 42°, I was by no means surprised to find that the thermometer belonging to the cork cup rose no higher.

The metal at 42° could not the water to the same temp.

In order to see if it could not be made to rise not only higher, but also more rapidly, by employing the metallic ball heated to such a temperature as it might by supposed would be sufficient to heat those particles of ice-cold water which should come into contact with its conical point, to the temperature at which the density of water is supposed to be a maximum, I made the following experiments.

A somewhat greater heat proposed to be applied.

Experiment. No. 2.

Having removed the ball, I gently brushed away the warm water, which, in the last experiment had been lodged in the cavity

Experiment 2. The former experiment was

repeated; the cone being heated to 60°.

cavity of the cork cup, (and which ftill remained there, as was evident from the indication of the thermometer belonging to the cup,) I placed feveral fmall cakes of ice in the cylindrical veffel, which ice floating on the furface of the water in the veffel, prevented that water from receiving heat from the air of the atmofphere, which at that time was at the temperature of 76° F. And as the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As foon as the cup, and the whole mafs of the water in the cylindrical veffel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the furface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the veffel, placing it exactly in the fame place which it had occupied in the foregoing experiment; but this ball, inftead of being at the temperature of 42° F. as before, was now at the temperature of 60° F.

The effect was more rapid and more confiderable.

The refult of this experiment was very ftriking, and if I am not much miftaken, affords a direct, unexceptionable, and demonftrative proof, not only that the maximum of the denfity of water is in fact at a temperature which is feveral degrees above the point of freezing; but alfo, that warm currents do actually fet downwards in ice-cold water, whenever a certain fmall degree of heat is applied to the particles of that fluid which are at its furface.

Particulars.

The conical metallic point had been in its place no more than ten feconds when I diftinctly faw that the mercury in the thermometer belonging to the cork cup was in motion; and, when 50 feconds had elapfed, it has rifen four degrees, viz. from 32° to 36°.

When two minutes and a half had elapfed, (reckoning from the moment when the metallic point was introduced into the cold water,) the thermometer had rifen to 39°, and at the end of fix minutes to $39\frac{7}{8}$°, when it began to fall; but very flowly however, for at the end of eight minutes and a half it was at $39\frac{1}{4}$°.

A thermometer near the outfide of the cup was not affected.

A fmall mercurial thermometer, the bulb of which was placed on one fide of the cork cup, at the diftance of about $\frac{2}{10}$ of an inch from it, fhewed no figns of being in the leaft affected by the heat communicated to the ice-cold water by the metallic ball.

This

This experiment was repeated three times the same day, (the 13th of June 1805,) and always with nearly the same results.

The mean results of these four experiments were as follows:

Tabulated results of the experiments repeated, with the the cone at a low heat.

Time elapsed reckoned from the beginning of the experiment. Min.	Sec.		Temperature of the water in the cork cup, as shewn by the thermometer.
0	0	- - - - - - -	32°
At 0	10	began to rise - - - -	32+
At 0	23	had risen to - - - -	33
0	28	- - - - - - -	34
0	35	- - - - - - -	35
0	48	- - - - - - -	36
1	3	- - - - - - -	37
1	35	- - - - - - -	38
2	32	- - - - - - -	39
3	41	- - - - - - -	39½
4	48	- - - - - - -	39¾
6	5	- - - - - - -	39⅞

As I had found by some of my experiments made in the year 1797, of which an account is given in my seventh essay, part I. that water at the temperature of about 42° F. and consequently what we should call very cold, melted considerably more ice, when standing on it, than an equal quantity of boiling hot water, in the same situation, I was very curious to see whether the thermometer, the bulb of which lay in the cork cup, would not also be less heated by the ball when it should be applied *very hot*, to the surface of the water, than when its temperature was much lower.

Whether a much greater heat in the metal would affect the water in the cup less.

To determine that point I made the following experiment.

Experiment. No. 3.

The cylindrical vessel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiously as possible taken out of that hot liquid, its projecting conical point was suddenly submerged in the ice-cold water, as in the former experiments.

Experiment 3. The experiment repeated with the cone heated to 212°.

The

The result of this experiment was very interesting, and it seems to me to throw much light on the subject of these investigations.

Effect on the thermometer in the cup much less.

It was not till 50 seconds had elapsed that the thermometer began to shew any signs of rising, and at the end of one minute and seven seconds it had only risen two degrees.

In the foregoing experiments, when the metallic ball was so much colder, the thermometer began to rise in ten seconds, and at the end of one minute and three seconds it had risen five degrees.

This difference is very remarkable; and if it does not prove the existence, and great efficacy of currents, in conveying heat in fluids, I must confess that I do not see how the existence of any invisible mechanical operation, the progress of which does not immediately fall under the cognizance of our senses, can ever be demonstrated.

As the experiment made with the ball heated in boiling water appeared to me to be very interesting, I repeated it twice, and its results were always nearly the same.

Tabulated results of experiments repeated; with the cone at a considerable heat.

The mean results of these three experiments were as follows:

	Time elapsed, reckoned from the beginning of the experiment. Min.	Sec.		Temperature of the water in the cork cup, as shewn by the thermometer.
	0	0	- - - - - - -	32°
At	0	50	the thermometer begun to rise.	32+
At	1	2	had risen to - - - -	33
	1	7	- - - - - - -	34
	1	18	- - - - - - -	35
	2	2	- - - - - - -	36
	3	2	- - - - - - -	36½
	4	17	- - - - - - -	37
	6	12	- - - - - - -	38
	7	17	- - - - - - -	38½
	9	0	- - - - - - -	38¾
	12	0	- - - - - - -	38¼
	14	0	- - - - - - -	38¼

Comparative view of the experiments.

By comparing the mean results of these experiments with the mean results of those in which the ball was at the temperature

rature of 60°, we may fee how much more rapidly the thermometer in the cork cup acquired heat when the metallic ball was *relatively cold* than when it was at the temperature of boiling water; and it is more than probable that it was not till after the conical metallic point had been confiderably cooled, by a contact with the ice-cold water, that thofe ftreams of moderately warmed water began to defcend from it, by which the thermometer was at length heated.

In the experiments made with the ball heated in boiling water, a fmall thermometer, placed juft under the furface of the ice-cold water, on one fide of the metallic point, began to rife very rapidly as foon as the hot ball was fixed in its place; but another thermometer placed about half an inch lower, on one fide of the cork cup, remained to all appearance at perfect reft, from the beginning of the experiment to the end of it.

When the very hot cone was ufed, a thermometer at the furface was heated; —but a lower one was not.

The explanation of all thefe appearances is fo eafy that it would be a wafte of time to fay much on that fubject. It may however be ufeful to recapitulate the principal phenomena, and fhew in what manner they tend to eftablifh the facts which they are brought to prove.

Explanation:

Every body muft fee, at the firft glance, that in all thefe experiments the heat which caufed the thermometer to rife was *carried* down into the cork cup by defcending currents of warm water; and it is evident that water which defcends muft of neceffity be fpecifically heavier than that in which it defcends.

That the flightly heated water defcends;

From the refults of thefe experiments we may conclude that the denfity of water is a maximum when that fluid is at a temperature fomewhat lower than that of the *fortieth degree* of Fahrenheit's thermometer.

—and the greateft denfity is rather below 40°.

In all the foregoing experiments, more or lefs warm water defcended through the ice-cold water into the cork cup; but the refults of the experiments which were made with the metallic ball heated in boiling water, fhew evidently that when the particles of water at or near the furface of a quiefcent mafs of ice-cold water, are by any means heated to a temperature feveral degrees above that of 40° of the fcale of Fahrenheit, fuch particles, fo heated, become fpecifically lighter than ice-cold water, and confequently cannot defcend in that cold and denfer liquid.

Hotter water is lighter and afcends.

With the hot cone some of the fluid ascended and some descended.

In the experiments in question, (with the hot ball) some of the particles of the water, namely, those which came first into contact with the conical metallic point, were heated to a higher temperature than that at which they were disposed to sink in ice-cold water; and these rose and spread themselves over the surface of the cold liquid; but others, which happened to acquire less heat, descended in it, and after filling the cork cup, overflowed, no doubt its brim, and then descending to the bottom of the brass cup, and coming into contact with that ice-cold metal, were there cooled, and there remained at rest.

Cork a good non-conductor.

As cork is an excellent non-conductor of heat, the warm water accumulated in the cork cup during an experiment retained its heat a long time after the heated ball was removed, notwithstanding its being surrounded on all sides, and even covered immediately by ice-cold water; (which by the by is a pretty strong proof that water is by no means a good conductor of heat) care however was taken, not only to remove this warm water after each experiment, by brushing it away with the soft end of a strong feather or quill, but also to cool the cup, and reduce it to the temperature of freezing water; which last operation was found to be much accelerated by brushing it out frequently with the feather. In order that this feather might itself be ice-cold, it was suffered to remain constantly in the ice-cold water, in the cylindrical vessel.

Cautions.

Apparatus by which the cone was fixed.

I must not forget to give an account of the means used for fixing the metallic ball in its place. This was done in a very simple and effectual manner. A slip of strong tin (G H) six inches long, and 2¼ inches wide, with a circular hole in the middle of it, one inch in diameter, being laid horizontally on the top or brim of the cylindrical vessel, in such a manner that the center of that circular hole coincided with the axis of the cylindrical vessel, the short cylindrical projection belonging to the ball being introduced into that hole, the ball was firmly supported in its proper place.

The quantity of ice-cold water in the cylindrical vessel was so regulated that the whole of the conical point being submerged, the surface of the water was on a level with the lower end of the cylinder, or, which is the same thing, with the base of the inverted cone.

When not only the whole of the conical point, but a part alſo of the ſhort cylinder were immerſed in the ice-cold water, the warmed water appeared to be thrown into eddies in its deſcent, which diſperſing about prevented its falling regularly in one continued ſtream into the cork cup.

The conical figure preferable for the immerſed metal.

To *conclude*, I would juſt obſerve, that although the foregoing experiments appear to me to be perfectly unexceptionable, and that their reſults afford demonſtrative proof of the facts which they were contrived to eſtabliſh; yet, when attempts are made by experiments ſimilar to theſe, to determine whether heat can be made to paſs *downwards* in water which is at a higher temperature than that at which its denſity is a maximum, difficulties occur which appear to me to be quite inſurmountable.

It is not eaſy to make concluſive experiments at elevated temperatures;

The fluidity of water is ſo perfect, or the mobility of its particles ſo great, that the liquid at the ſurface, which is firſt heated and rarefied, immediately ſpreads far and wide, and meeting with the ſides of the containing veſſel, heats them, and this heat, ſo acquired, making its way downwards (as well as upwards) in the ſolid ſubſtance of which the veſſel is conſtructed, raiſes the temperature of the lower ſtrata of the fluid in contact with it, which moving towards the axis of the veſſel, communicates heat to a thermometer placed there, below the ſurface of the water.

For the ſides of the veſſel greatly modify the effects by conducting the heat, &c.

That theſe various operations do in fact take place, nobody can doubt: and it appears to me to be the more probable that all the heat which a thermometer placed below the ſurface of warm water acquires when a great degree of heat is applied to the particles of that fluid which are at the ſurface, is in fact received from the ſides of the containing veſſel, not only becauſe the thermometer acquires heat ſo very ſlowly, but alſo, and more eſpecially, becauſe this heat is acquired much more ſlowly when the containing veſſel is wide than when it is narrow; and alſo when it is made of a ſubſtance which is a good conductor of heat than when it is conſtructed of a ſubſtance which is a bad conductor of heat, as I have found by experiment. But as this particular enquiry is foreign to my preſent purpoſe, I ſhall not enlarge on it in this place.

Facts which ſhew the probability that conſiderable heats are chiefly tranſmitted downwards through fluids by the agency of the veſſel.

Deſcription

II.

Description of a Press for preserving botanical Subjects; with an Account of the Success of the Improvement in the Art of Blasting pointed out by Mr. JESSOP *in this Journal. In a Letter from Mr.* THO. HARRISON.

To Mr. NICHOLSON.

SIR,

Introductory obs. on preserving plants.

BEING desirous to make a collection of the plants which grow in the neighbourhood of Kendal, and to preserve them in an herbarium, I was naturally led to a consideration of the different methods, which have been used by botanists, of drying and preserving specimens; but finding none so complete and expeditious as I could wish, I therefore adopted a plan of my own, which I am persuaded will be found to answer every purpose that the botanist can require. The real utility which is to be derived by a young botanist from the preservation of plants, is the power of future examination, and to answer the purpose the natural appearance of each specimen ought to be preserved as calculated to produce this effect; for since the seed-vessels and stems of plants occupy much more space in thickness than the leaves, in order to preserve the latter from shrivelling, the two former must generally be bruised in such a manner as totally to preclude any accurate investigation afterwards: and this on trial is found to be the case: besides, all the plants that happen to be in the press at the same time, however various their texture, are subjected to the same degree of pressure.

Remarks on the methods now in use.

The plan recommended by T. Velley, Esq. seems equally exceptionable; I mean the method of placing the plant when fresh between several sheets of blotting-paper, and ironing it with a large smooth heater pretty strongly warmed, till all the moisture is dissipated. By this method it is evident that the parts of fructification must be much bruised: it may preserve the colour of the blossoms better than any other, but this in the science of botany is not very essential, at least if it was, I believe that no method hitherto discovered will succeed universally in arresting the fading beauty of delicate flowers. The next plan which is given by Dr. Withering, is one used by

by a Mr. Whateley a ſurgeon, but his contrivance, though certainly better adapted to preſerve the parts of fructification and the ſhape of the ſtem entire than the two former, I found would take up much more time than a country ſurgeon can well ſpare; therefore this led me to make what I thought an improvement on Mr. Whateley's plan, and the experienced botaniſt is left to judge of my ſucceſs from the following deſcription; this I can ſay for it, that it anſwers completely (of courſe) to my own wiſh; but further, that Mr. John Gough, of Middleſhaw, who has the beſt collection of plants in this county, regrets that he had not the ſame contrivance, and has urged me much to ſend you the following deſcription of my preſs.

This inſtrument conſiſts of 17 oblong boxes, each, excepting the uppermoſt and lowermoſt, is made of four ſides of well-ſeaſoned oak wood, two inches deep and one-fourth of an inch thick, dove-tailed together; the two end-pieces of which have two notches each in the middle wherein to place the fingers in lifting it; and the bottom conſiſts of canvas glued and nailed to the wooden frame: in each corner is fixed a ſmall triangular piece of wood reaching half an inch from the bottom upwards (for a purpoſe hereafter to be explained). Theſe boxes are made ſo as to be placed one within another ſucceſſively upwards, the loweſt (I am ſpeaking only of the 15 with canvas bottoms) meaſures 20 inches by 16 on the outſide, and the higheſt 12 inches by 8. The bottomeſt, or carriage on which all the others are ſupported, is much ſtronger, the ſides of it being two inches deep and three quarters of an inch thick: its bottom is of wood of the ſame thickneſs, all over perforated with holes one-third of an inch in diameter, and reſts on four iron caſtors, one at each corner, to render the whole more eaſily moveable; at each end of it there is an iron handle: This box is 20 inches by 16 within, and will therefore barely receive the largeſt of the boxes with canvas bottoms. The uppermoſt, or 17th box, is of the ſame conſtruction as the canvas boxes, excepting that its bottom is of wood of the ſame thickneſs as its ſides, with a number of holes pierced through of the ſame magnitude as thoſe in the carriage, and canvas glued to the under ſide of it. To complete this preſs, two folds of blotting-paper are to be placed

The author's new preſs deſcribed. It conſiſts of a ſeries of trays or boxes, having (all but the loweſt) canvas bottoms. They contain ſand, and the plants are laid between box and box.

placed over the holes in the bottom of the carriage, in which, as well as all the canvas-boxes, must be put an inch in depth of the finest sand, washed and sifted; river sand is most eligible, as being least angular; which sand must be made level on the top by drawing over it a rule that reaches one inch into each box: these boxes are placed one within another, and each by containing an inch depth of sand, raises the incumbent frame one inch, so that when they are thus charged and placed one upon another, the whole makes a truncated pyramid of 18 inches in height, and weighing about 1 cwt. The uppermost box may be filled with sand, or, if more weight be necessary, with shot. To this box I have added (what is by no means essential) a cap, consisting of the sides of three boxes fixed one within another, each projecting one inch above its inferior, for the sake of uniformity; the top is covered by a thin board: when this cap is placed upon the uppermost box, the whole has not an inelegant appearance.

Reference to the engraving.

Lest the description should not be sufficiently clear, I have sketched the following figure, representing a perpendicular section of the press viewed on one side of it. *Plate XV.*

Fig. 1. represents a perpendicular section of the press made lengthwise.

a. The end of the bottomest box or carriage.

b. Its bottom part perforated with holes.

c. The end of the first box with a canvas bottom.

d. The canvas bottom.

e. The sand which but half fills the box.

f. The 17th or uppermost box, with a perforated wooden bottom.

g g g. Represent the end of the cap formed of the frames of three boxes, in the empty space of which may be placed the botanist's pocket box.

Fig. 2. represents the rule wherewith the sand is made level in the boxes; it is 18 inches long.

h. A notch one inch deep.

i. The edge made thin.

k. A notch an inch long.

Fig. 3. represents the end of a canvas box.

l l. The two notches to place the fingers in when the box is to be lifted.

The

The Method of uſing it.

Method of uſing it. The plants are laid between blotting-paper, and the preſſure regulated by the number of boxes above each, &c.

After a ſpecimen is ſelected, a ſlip of paper is to be attached to it, containing its name, place of growth, and time of gathering. The boxes are then to be taken off by three or four at a time, until the number removed may be judged to be of ſufficient weight to produce the neceſſary preſſure. On the ſand in the box thus ſelected for uſe, a ſingle fold of blotting-paper muſt be placed; upon that the plant, taking care to preſerve as much as poſſible the natural poſition of its characteriſtic parts: Over the plant a ſecond fold of paper muſt be laid, and then the boxes are to be replaced. If the plant be woody and require much preſſure, place it near the bottom; but if ſucculent, near the top; and herein conſiſts one great advantage of this preſs over every other, ſince the preſſure can be ſelected or varied at pleaſure, and all the parts of the plant preſſed equally; for if the plant be ſucculent, the preſſure can ſcarcely be too ſlight at firſt, and may be gradually increaſed without ever bruiſing the tendereſt part; and if, on the other hand, ſtrong preſſure be required, the plant may be placed in the bottomeſt box; and if the weight of ſand be not thought ſufficient, any number of the boxes next above it may be loaden with ſmall ſhot, by which the preſſure may be increaſed until the boxes be full, when the preſſure exerted would be much greater than the ſtrongeſt plants could bear without having ſome of its eſſential parts defaced. Another advantage which led me to give a preference to this method of preſſing plants, was the great ſaving of time; for though my profeſſional avocations ſhould require my immediate preſence at a diſtance, and thus drag me from this innocent amuſement when in the buſieſt part of claſſing and arranging my ſpecimens, and though my preſs ſhould be completely ſeparated into its conſtituent parts, yet the whole can be arranged, in the ſpace of one minute, to remain until the next leiſure time that may occur; ſo that, in fact, with this contrivance, botany is a ſtudy that can be entered into or given up as it may ſuit the convenience of a moment; which is not the caſe where any of the plans deſcribed in Dr. Withering's Introduction to the Study of Botany * are followed.

* Third Edition.

It

Remarks on the effects and advantages of this contrivance.

It may be neceſſary to explain ſome parts of the ſtructure of the preſs. The holes in the bottomeſt and uppermoſt boxes are to admit of the circulation of air and the evaporation of moiſture, which will be conſiderable when the preſs without its cap is placed within the influence of the ſun or fire, as it always ought to be: and the ſmall triangular pieces of wood in the corners of each box, are to prevent the upper boxes from preſſing ſolely on the canvas bottom of the loweſt box, when any number are lifted together: theſe may at firſt ſight appear likely to impede the preſſure of the ſand when placed in the preſs, but this objection will vaniſh when it is recollected that each box contains an inch depth of ſand: and it is evident, on examination of the preſs, that the ſides of the boxes ſerve no other purpoſe than to keep the ſand together; for they are perceptibly moveable in a perpendicular direction, and ſuſtain no preſſure from the ſuperior boxes; therefore the plant is placed as if in a heap of ſand.

The beſt method of preſerving dried plants is to ſtitch them to a paper.

I am afraid I have already treſpaſſed too long on the attention of the reader, but I cannot diſmiſs the ſubject without offering a few obſervations on the manner of preſerving plants after they are dried. There have been at leaſt three methods uſed: firſt, fixing them to paper by the aid of gum-water: ſecond, placing them looſely in a book: third, ſtitching them with thread to a ſheet of paper. The firſt plan appears to me the leaſt eligible, for it cannot be neatly executed unleſs the plant be preſſed nearly flat, and by fixing it to the paper it can never admit of an accurate examination in future, but becomes a mere picture, and a very imperfect one; for almoſt every diſtinguiſhing character, eſpecially the parts of fructification, are injured or defaced; even the general habit is often ſo much altered that the plant could not be aſcertained, were it not for the name generally ſubſcribed at its root. The ſecond plan is alſo objectionable; for though the plant may at any time be ſubjected to examination, yet by lying looſe in a book, it is very apt to be broken by the leaſt motion, particularly when kept dry, which all preſerved plants ought to be. The third appears to me the leaſt liable to objection; for if the plant be neatly ſtitched down, it will not be ſubject to ſuch motion as to injure its parts; and by cutting the threads

threads it may be detached from the paper, and subjected to any future investigation.

I am, Sir,

Your's, &c.

Kendal, July 8, 1805.

THOMAS HARRISON.

P. S. I cannot refrain from calling to the recollection of your readers a valuable paper in the 9th Vol. p. 230, of your Journal, recommending the use of sand in the blasting of rocks. The effects there related of this simple agent, I confess did astonish me more than any thing I ever read: and as very serious and even fatal accidents have happened by the method of ramming blasts with stone in the limestone and slate quarries in this neighbourhood, I was determined to repeat the experiment the first opportunity, and thus shew its effects to the men who work in those places. Accordingly no long period elapsed before I had occasion to be in the neighbourhood of the slate quarries in Longsliddale, about ten miles from Kendal; when I went to one of those quarries, taking with me a bag of small sand, consisting of powdered freestone used in this country to scour pans with, and strew upon the stone floors. I mentioned my business to one of the workmen who was then engaged in boring a hole 24 inches deep and about an inch diameter in a slate rock; the stratum was about 27 inches thick, and reclined from the perpendicular about 20 degrees; and this being the first blast, it was consequently firmly surrounded and fixed in on all sides by solid rock: The part that he wanted to throw out by the blast was supposed to be five ton weight: The direction of the hole which he was boring was perpendicular to the stratum, and therefore elevated above the horizon about 20 degrees. He smiled at my proposal, but said he would try the experiment to satisfy me, provided I would pay for the powder if it failed; but he seemed to think that I had a very poor idea of blasting, to believe that a little light sand would answer the purpose of the laborious and dangerous process of ramming which he had been accustomed to use. However, he charged his blast with powder in the manner he had been used to do; *i. e.* into this hole of

Account of the successful application of the method of blasting rocks indicated in this Journal by W. Jessop, Esq.

Safe and easy method of blasting rocks.

of 24 inches in depth, he put as much powder, together with two or three pieces of broken sticks, of half an inch diameter and about four or five inches long, as reached 12 inches upwards: The sticks he introduced for the purpose of keeping the powder loose in the hole, so that it might take fire more nearly all at the same instant: This done, he put in a charged straw, into the outer end of which was fixed a match-paper; and lastly, he (with a smirking countenance, anticipating the joy he expected to feel at my disappointment) filled up the hole with sand. The match was then fired, upon which we removed to a distance, and the powder soon exploded; when he immediately exclaimed, from the peculiarity of the report, though the effect was unseen by him, that the rock was as completely shattered as if the hole had been rammed with stone; and a view of the place verified the prediction. Since that time the plan has been adopted in that and most of the neighbouring quarries, with the use of small sand which is washed out of the river in floods, and the method answers as well as the old one which was attended with so much danger. In one case the sand was thrown out twice without any effect on the rock; the third time, the hole was rammed in the usual manner with stone, which was thrown out likewise; so that the new process fortunately lost no reputation.

I have detailed thus much in confirmation of the valuable paper before mentioned, and in hopes that it may induce some of your readers who reside in the neighbourhood of mines or quarries, to endeavour to introduce this safe and easy method in lieu of the dangerous and tedious one of ramming with stone, in which process we have had repeated instances of the loss of limb, vision, and even life itself: In the use of sand there is comparatively no danger, and by adopting this plan many a valuable life will be saved.

Description

III.

Description of a Safety Valve, containing a Vacuum Valve in the same Hole of the Boiler. By Sir A. N. EDELCRANTZ.*

IN large boilers or coppers, where boiling fluids are enclosed, a *safety valve* is generally used to prevent their bursting, from an unexpected excessive force of the elastic steam, and, besides, a *vacuum valve*, to prevent their being compressed or crushed by the weight of external air, in the case of a sudden condensation of the vapours. These two valves are commonly fitted in two different holes in the boiler; but as a more simple, and consequently more eligible, method seems to be that of joining them together, I take the liberty to submit to the Society for the Encouragement of Arts, &c. the following contrivance for that purpose:— Large boilers require a safety valve and a vacuum valve.

a b, Plate XII. Fig. 2. is a common conical safety valve, fixed in the boiler *c d*, having four openings, *i i*, which are represented in a plan view in *Fig* 3: *e f* is the metallic rod, bearing the weight K K, with which the safety valve is loaded, and extending itself under that valve to *f*: *g h* is the *vacuum valve* consisting in a plane circular plate, with a brass tube sliding along the rod, and pressed by a spiral spring to the safety valve *a b* (against which it has been well ground in making it), closing in that situation the openings *i i*. Description of a valve that performs both offices.

Such being the construction of the whole, it is evident, that when the elasticity of the steam increases, the two valves, joined together, with the holes *i i* shut, make but one, opposing to the elasticity of the steam an united resistance, which is regulated by the weight *k k*, in the common way; but, on the contrary, when by condensation of the vapours a vacuum is produced, the external air in pressing through *i i*, upon the vacuum valve *g h*, forces it down, and opens to itself a passage into the boiler. It is a loaded valve opening upwards; having in its plate a spring-valve opening downwards.

The valve *g h* may easily be made conical, like the other, if that form should be preferred; but in different trials, I have found planes, if well turned and ground together, join as per-

* From the Memoirs of the Society of Arts for 1804. The silver medal was awarded for this invention.

fectly as can be desired, being pressed by the united elasticity of the spring and the steam.

The same applied to a former contrivance.

Fig. 4. is the same contrivance adapted to a new kind of *safety valve* or *piston*, which, though I originally intended it for the use of Papin's digesters of a new construction *, has been, in a larger size, applied by me even to steam engines, and is described in the Philosophical Magazine of December, 1803.†

Other experiments.

I have lately begun, and shall pursue, a set of experiments, with the intention of regulating by this safety piston, the quantity of admitted air to fire-grates, and to effect, by that means, a new mode of regulating the fire, and the elasticity of steam in boilers, with less expenditure of fuel and of force than usual; of which idea a hint is given in the work and place above mentioned. The result of these researches I shall at some future period do myself the honour of communicating to the Society.

IV.

Observations and Experiments for the Purpose of ascertaining the definite Characters of the primary Animal Fluids, and to indicate their presence by accurate chemical Tests. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

Great want of precision in the animal analysis; particularly of the fluids.

THE precision which the analysis of mineral and vegetable substances has attained, does not appear to be yet extended to

* Nicholson's Journal, March, 1804.

† The description of this contrivance being already published, it would be superfluous to repeat it. I only beg leave to add the following practical remark. A metallic piston, if well turned and fitted into a cylinder of exactly the same kind of metal, will probably have the same degree of expansion, especially if hollow, and consequently will not increase its friction in any increased degree of temperature. But as in practice the cylinder is commonly exposed to a lower temperature than the piston, heated by the steam, a little increase of friction will take place by an increase of heat. To prevent the effect of this, I have found it useful to employ for the piston a metal of somewhat less expansive power than the cylinder; and the expansion of red copper being to that of brass nearly as 10 to 11, I prefer making the piston of the former metal, when the cylinder is made of brass.

the

the products of the animal kingdom. This remark may be applied both to the solids and fluids which compose the animal body, but it is the most applicable to the latter class of substances. The terms serous, mucilaginous, gelatinous, &c. are employed, even by the most esteemed medical and physiological writers *, in a vague and indeterminate manner, without attending either to the original import of the word, or to the restricted meaning, which it is necessary to impose upon popular expressions when they are adopted in scientific researches. The object of the present paper is to ascertain a definite character for what I propose to call the primary animal fluids, and to discover delicate and accurate tests, by which their presence may be easily and certainly indicated. By primary animal fluids, † I mean those into which the compound fluids existing in the animal body are capable of being resolved by the application of different re-agents, without decomposing them into their ultimate elements.

Primary animal fluids.

Albumen.

The first of the animal fluids which I propose to make the subject of my investigation is the albumen. With the exception of water, no fluid appears to enter so largely into the composition of the animal body. It forms a very considerable proportion of the blood, and is found in greater or less quantity in nearly all the secretions. It is also capable of assuming the solid form, without undergoing any change in its chemical properties; in this state it constitutes the basis of all the membranous substances, which are so extensively dispersed through every part of the system; it composes the cellular tissue into which the earth of the bones, and the fibrous matter of the muscles are deposited, while it enters largely into the structure of the skin, the glands and the vessels. At present, however, we shall direct our attention to it while in the liquid form.

Albumen, a large component part of animals.

* Even Mr. Abernethy, in his late valuable work on tumors, speaks of the gelatinous part of the blood, where I conceive from the context, there can be no doubt that he intended to designate the fibrine.

† All the animal fluids, both primary and compound, are merely solutions of a solid body in water; but those substances which are most frequently seen in a state of solution, have very generally obtained the title of fluids.

In

Most conveniently obtained from white of egg.

In order to obtain it in a state of purity, I had recourse to the white of the egg, a substance to which the name of albumen was originally applied, and which is still considered by the most eminent chemists * as composed entirely of this substance. In order to ascertain how far this opinion was correct, I kept a quantity of the white of the egg in a temperature of 212°, until it was firmly coagulated. It was then cut into small pieces and placed in the upper part of a narrow-necked funnel, when a few drops of a brownish viscid fluid were separated from it. Other pieces of the same coagulum were kept for some time in boiling water, the fluid being then passed through a filtre, had acquired a light brownish colour, and a faint odour; when agitated it was slightly mucilaginous. By slow evaporation, a small quantity of a brittle, semi-transparent substance was obtained. It appeared therefore evident, that the white of the egg contains a small quantity of a substance incapable of coagulation, and therefore essentially different from albumen. I still, however, continued to employ it for the purpose of ascertaining the properties of albumen, as it affords this substance in a state of greater purity than it can be obtained from any other source.

White of egg is not pure albumen; it contains a little of matter not coagulable.

White of egg contains 80 parts water; 15,5 solid albumen and 4,5 uncoagulable matter.

It was an object of some importance to ascertain the proportion which this foreign ingredient bore to the albumen itself; 100 grains of firmly coagulated albumen were kept for some time in boiling water, and this being poured off, a fresh quantity was added, and this process repeated until the water appeared to contain no farther impregnation. The whole of the fluid was then evaporated, and a residuum was left which amounted to $4\frac{1}{2}$ grains. Besides the admixture of this peculiar substance, the white of the egg contains a considerable quantity of water, not only in its liquid state, but after it is coagulated. By a gentle heat the water may be evaporated, and the solid matter is left behind in the form of a hard, brittle, transparent substance; I have found that upon an average, $\frac{4}{5}$ of recently coagulated albumen may be considered as consisting of water. Hence it will appear, that 100 grains of the white of egg consist of 80 grains water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

* Hatchett, Phil. Transf. 1800, p. 375.
Thomson's Chemistry, IV. p. 484.

The

The most distinguishing characteristic of albumen is the property of being coagulated by heat, which forms an obvious and easy test of its presence, when it exists in a compound animal fluid in any considerable proportion. In order to ascertain how small a quantity of albumen could by this means be rendered visible, I added 13 grains of the white of the egg to 87 grains of water, and thus formed a solution, one grain of which contained $\frac{1}{10}$ grain of pure albumen. Five grains of this solution were then added to 95 grains of water, so that 100 grains of water contained $\frac{1}{10}$ grain only, or $\frac{1}{1000}$ of its weight of pure albumen. This was exposed to the heat of boiling water, and a perceptible opacity was produced in the fluid. Distinguishing character of albumen coagulated by heat. One thousandth part in water will coagulate;

The effects of the oxymuriate of mercury were next tried. One drop of a saturated solution of this salt, added to 100 grains of water, containing $\frac{1}{1000}$ of its weight of albumen, produced a very evident milkiness; after some hours a curdy precipitate separated and fell to the bottom. A solution of half the strength, containing only $\frac{1}{2000}$ of its weight of albumen, was then tried by the same re-agent, and even in this instance, a sufficiently obvious effect was produced. —oxymuriate of mercury precipitates it;

The nitro-muriate of tin is a powerful coagulater of albumen in its unmixed state, but I found it not to be so delicate a test as the oxymuriate of mercury. One hundred grains of water, containing $\frac{1}{5}$ grain of albumen, i. e. $\frac{1}{500}$ of its weight, was not affected by this re-agent until after some hours, when the fluid exhibited a degree of milkiness. —as does nitro-muriate of tin,

In order to ascertain the effects of tannin upon albumen, I macerated half an ounce of powdered galls in half a pint of water for some hours, and filtered the fluid. A deep brown transparent liquor was produced, 100 grains of which I found by evaporation to contain $2\frac{1}{2}$ grains of solid residuum. Equal parts of this preparation of galls and of a solution of albumen, in the proportion of one part to 1000 parts of water, were mixed together; at first no effect could be perceived, but after some time an evident precipitate was formed and slowly subsided. —and tannin.

The aqua lithargyri acetati, or extract of Goulard, is an active precipitant of several animal fluids; when dropped into a strong solution of albumen, a very copious and dense precipitate is immediately formed. Goulard throws it down;

It

—but doubtfully.

It is, however, somewhat difficult to determine how far this effect depends upon the albumen itself, as Goulard has been considered to be the appropriate test of the uncongelable part of the serum of the blood, which, it may be supposed, resembles the uncoagulable part of the white of the egg. The aqua lithargyri acetati is likewise decomposed by several of the saline bodies which are found to exist in almost all the animal fluids. I have sometimes found it to yield a precipitate even when added to distilled water, and in all cases, after exposure to the atmosphere for a few hours, the water to which it has been added becomes turbid, and is covered with a thin film. In order to try the effect of this re-agent on albumen, I added one drop of it to 200 grains of water, and upon observing that the transparency of the fluid was not affected, a single drop of the solution of albumen, of the same strength with that mentioned above, was added. It formed a dense precipitate as it fell through the fluid, and upon agitation, the whole was rendered slightly milky. In this case the proportion of the water to the albumen was as 10.000 to 1; to the uncoagulable part of the white of the egg, it would be about as 30,000 to 1.

Aqua lithargyri acetati plentifully precipitates it.

Nitrate of silver also has a like effect;

The next re-agent which I employed was the nitrate of silver. A single grain of a saturated solution of this salt produced an evident turbidness in 100 grains of water, containing $\frac{1}{10}$ gr. of albumen, and after some hours a small quantity of a curdy precipitate fell to the bottom of the vessel. It might, however, be suspected that in this case, the effect produced depended upon a quantity of muriate of soda contained in the albumen. I found that 100 grains of water, containing only $\frac{1}{100,000}$ gr. of common salt was rendered evidently turbid by one drop of the nitrate of silver; * but the precipitate which is

* I weighed very exactly a grain of salt, and dissolved it in 200 grains of water. One grain of this solution was afterwards added to 99 grains of water, and by repeating the operation for three successive times, I obtained 100 grains of water, containing only $\frac{1}{10,000}$ of its weight of salt. I then took 99 grains of distilled water, and poured into it one drop of the nitrate of silver: after waiting for some time, until I was satisfied that no effect would take place, I added a single drop of the last solution of salt, thus making it $\frac{1}{100,000}$ part of the mixture; a faint but perceptible opacity was almost immediately produced.

is formed by the nitrate of silver acting on the muriate of soda is in the form of a greyish powder, and subsides more rapidly than it did in the former instance, where it produced a white, flaky precipitate.

I found that a solution of albumen of the same strength with that employed in the last experiment, was immediately decomposed by the nitro-muriate of gold. One drop of this metallic solution instantly produced a dense white precipitate in 100 drops of water, containing only $\frac{1}{10}$ of a grain of albumen. —and nitro-muriate of gold;

Albumen in a concentrated state is powerfully coagulated by alum; I found however, that this re-agent is not so accurate a test of its presence when in a diluted state, as some of those which I had already employed: $\frac{1}{5}$ grain of albumen, dissolved in 100 grains of water, was indeed rendered slightly turbid by the addition of a few drops of a saturated solution of alum, but no precipitate was formed. —and alum.

Before I conclude my account of these experiments I must observe, that the strength of the solution of albumen was in all cases rather less than my estimate. When I added the albumen to the water, a small portion of it always remained insoluble, and this was separated from the fluid by filtration before the experiments were performed. This insoluble part I supposed to consist of the membranous matter, with which it is said that the white of the egg is interfected. The quantity was indeed almost too small to be appreciated, but where it is desirable to attain as much accuracy as possible, I think it necessary to mention every circumstance which may in the smallest degree affect the result. Obs. on the solution of albumen.

The experiments related above will, I conceive, indicate with a sufficient degree of accuracy, the presence of albumen as a constituent of an animal fluid. The property of being coagulated by heat is a characteristic of this substance, which will always serve as a mark of discrimination, and we have found that this property is not destroyed by dilution with 1000 times its weight of water. This therefore may be considered as a test of its presence minute enough for all practical purposes. We have also found that there are several re-agents which possess the power of precipitating it from its solution in water, while existing only in the same proportion. It will be necessary, however, to observe their operation upon the other animal substances before we can determine their use in the analysis or compound fluids. Remarks. The coagulation by heat is a good distinctive character of albumen.

Jelly.

Jelly.

Jelly. It is liquifiable by heat and becomes concrete by cold; and is a large component part of animals.

The next fubftance which I propofe to examine is jelly. The peculiar characteriftic of this body is its property of becoming concrete by cold, and being liquified by the application of a gentle heat. It enters into the compofition of the blood, though lefs largely than the albumen. It is alfo an ingredient in the fkin, membranous texture, ligaments, cartilages and tendons. By boiling it is eafily extracted from thefe fubftances, and by evaporation and cooling the whole is reduced to a mafs of greater or lefs folidity, in proportion to the previous degree of concentration. By a procefs of this nature, ifinglafs is prepared from the bones and cartilages of fifh; as this fubftance has been confidered to confift of jelly nearly in a ftate of purity, I employed it for the following experiments.

Obtained by the folution of ifinglafs. One fiftieth part in water will coagulate.

Four grains of ifinglafs were diffolved in 200 grains of water, and thus a ftandard fluid was formed, one grain of which contained $\frac{1}{50}$ gr. of jelly. This folution became perfectly concrete by cooling. In the firft place I wifhed to afcertain how fmall a proportion of jelly diffolved in water was capable of affuming the concrete ftate. Equal parts of the ftandard fluid and of water, *i. e.* one part of jelly to 100 parts of water, produced a compound which was completely ftiffened by cooling; but I found that two parts of water to one of the ftandard, *i. e.* one part of jelly to 150 parts of water, produced a compound, which though evidently gelatinous, did not affume the concrete form.

It is actively precipitated by tanin.

One of the moft active precipitants of jelly is the tanning principle. I found that a mixture of 5 grains of the ftandard folution and 95 of water, produced a copious precipitate when added to an equal quantity of an infufion of galls of the fame ftrength with that employed in the experiments upon albumen. In this inftance the jelly was to the water as 1 to 1000. I afterwards reduced the quantity of jelly until it compofed $\frac{1}{5000}$ part only of the folution, and in this cafe a confiderable precipitate was ftill produced by the infufion of galls.

—but not by aq. lith. acet.

A quantity of the ftandard folution had a few drops of the aqua lithargyri acetati added to it, but no more effect appeared to be produced than would have enfued from mixing it with an equal quantity of pure water.

2 No

No effect was produced by adding the oxymuriate of mercury to the standard solution. The nitrate of silver and the nitro-muriate of tin were each employed, and produced a very slight and almost imperceptible opacity. —nor by ox. m. of merc. nor n. of silv. nor n. m. of tin.

The addition of the nitro-muriate of gold caused a small quantity of a dense precipitate when added to the standard solution; but when this was so far diluted as to contain one grain of jelly in 500 of water, the effect was no longer perceptible. N. m. of gold gave precip.

Mucus.

Animal mucus or mucilage enters largely into the constitution of many parts of the body, and forms a considerable proportion of several of the secretions. The term mucus had been generally employed in a vague and unrestricted sense, until Mr. Hatchet, in his valuable paper on the membranous parts of animals, inserted in the Phil. Transf. for 1800, attempted to assign to it a more appropriate and definite meaning. He conceives that jelly and mucus are only modifications of the same substance, and do not essentially differ from each other; he considers it to be entitled to the appellation of mucus, when it is soluble in cold water and cannot be brought to the gelatinous state *. Dr. Thomson adopts in general the idea of Mr. Hatchett, and lays down the following as the characteristic properties of animal mucilage. It is soluble in cold water, insoluble in alcohol, neither coagulable by heat nor generating into a jelly, precipitable by tan and the nitro-muriate of tin †. I have been induced from the results of my observations, to form a different opinion respecting the relation of jelly and mucus, but I shall defer the statement of it until I have related the experiments which have led me to dissent from such high authority. Mucus; a term not distinctly applied by chemists.

By agitating for a short time some recent saliva in cold water, part of it was dissolved, and after being passed through a filter, was made the subject of experiment, being, as I conceived, a solution of nearly pure mucus. By a careful evaporation I found that the water had dissolved $\frac{1}{240}$ part of its weight. Mucus obtained from saliva.

* Hatchett, Phil. Transf. 1800. 369 and 381.
† Thomson, IV. 503.

No

Effect of reagents. No coagulation by moderate heat nor did it gelatinise by cooling.

No effect was produced by the addition of the oxymuriate of mercury, and the nitro-muriate of tin caused only a slight opacity. No effect was produced by the addition of equal parts of this solution and the infusion of galls. The aqua lithargyri acetati added to the solution produced an immediate opacity, and after some time a white, fleaky precipitate fell to the bottom of the glass. No appearance of coagulation was produced by exposing the fluid for some time to the heat of boiling water, nor was there any tendency to gelatinize by evaporating and afterwards cooling the fluid.

Mucus from an oyster.

I afterwards endeavoured to obtain mucus in a state of purity from another source. For this purpose an oyster was agitated for a few minutes in cold water; the fluid was filtered and appeared slightly opake and glutinous.

By evaporation it appeared that it had dissolved about $\frac{1}{50}$ of its weight of animal matter. A quantity of this solution, diluted with an equal bulk of water, was employed in the following experiments.

Reagents.

The oxymuriate of mercury being added to it produced no effect. The infusion of galls after some time produced a slight degree of turbidness, and at length a precipitate was formed in small quantity. The aqua lithargyri acetati caused an immediate opacity, and after some time a dense precipitate.

The Goulard indicates its presence.

These experiments nearly coincide with the former. In both cases no effect was produced by the oxymuriate of mercury, thus proving the absence of albumen. The small precipitate caused by the galls indicate the existence of only a very minute quantity of jelly. The effect was scarcely as great in this instance, where the animal matter composed $\frac{1}{100}$ part of the weight of the solution, as was produced by the same reagent upon a solution of jelly, where it composed only $\frac{1}{5000}$ part of the weight of the fluid. Very nearly the whole therefore of the animal matter probably consisted of mucus, the presence of which was indicated by the Goulard.

Exclusive tests; tan for jelly, aq. lith. acet. for mucus, ox. m. of merc. for albumen.

I apprehend that these experiments will be deemed sufficient to establish a decided and essential difference between mucus and jelly, independent of the gelatinizing property of the latter, the effects produced upon them by the tanning principle and by the aqua lithargyri acetati are exactly opposite. Tan is a most delicate test of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate test of mucus

cus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the most accurate tests of albumen, does not appear to be affected either by jelly or by mucus.

Albumen, jelly, and mucus, I am inclined to consider as the only primary fluids which are dispersed through the different parts of the body. Particular vessels or glands contain and secrete particular fluids, which cannot be resolved into other fluids without decomposition, as the fibrine of the blood, the resin of the bile, the urée of the kidney, &c. but these are in all instances confined to their appropriate organs, and do not necessarily enter into the present investigation. These are the primary and general fluids.

From the above experiments I think we may be entitled to lay down, with a considerable degree of accuracy, the leading characteristics of the three primary animal fluids, and to establish tests by which their presence may be minutely ascertained. The most remarkable property of albumen is its becoming coagulated by heat, a property which it retains so far as to communicate a degree of opacity to its solution in water, when it forms only $\frac{1}{1000}$ part of its weight. A solution of the same strength has its albumen precipitated by the oxymuriate of mercury, and this test will indicate its presence when composing no more than $\frac{1}{2000}$ of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of silver, and the nitro-muriate of gold, are all tests of the presence of albumen nearly as minute as the oxymuriate of mercury, but they are less valuable, because their effects are not confined to albumen. The nitro-muriate of tin and alum are also precipitants of albumen, but they are less delicate in their operation than the reagents enumerated above. Resumption. Albumen is known by its coagulability, and precip. by ox. m. merc.

The peculiar characteristic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat: we have found that its solution in water retains this property when it composes $\frac{1}{100}$ part only of the weight of the fluid. Tan is a still more minute test of jelly than of albumen, but jelly is not in the least degree affected by the oxymuriate of mercury, and may thus in all cases be easily distinguished from it. No effect is produced in jelly by Goulard, and scarcely any by the nitrate of silver, and the nitro-muriate of tin, when it is in a state of such dilution. By means of tan, jelly may be easily detected in a fluid of which it forms only $\frac{1}{5000}$ part. Jelly is known by its concretion on cooling and its precip. by tan.

Mucus is negative as to the preceding characters; but precip. by aq. lith. acet.

The properties of mucus are principally negative; it is not coagulable by heat, nor capable of becoming gelatinized; it is not precipitable either by the oxymuriate of mercury or by tan, but it may be detected with considerable minuteness by the aqua lithargyri acetati.

Other tests.

It appears therefore that the oxymuriate of mercury, tan, and the aqua lithargyri acetati are the three most valuable tests. The nitro-muriate of tin is a less delicate test of albumen than the oxymuriate of mercury, and is also in some degree affected by jelly. The nitrate of silver appears to be a very nice test of albumen, but it is objectionable in consequence of its being decomposed by the muriate of soda, a salt which is supposed to exist in most of the animal fluids. The nitro-muriate of gold is a delicate test of albumen, but it likewise precipitates jelly.

Order of analysis.

In the analysis of a fluid which is supposed to contain either albumen, jelly, or mucus, the first step is to observe the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in question contains no albumen. We must next employ the infusion of galls, and if this also cause no precipitate, we may conclude that the animal matter held in solution consists of mucus alone.

Remark.

I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which these substances bear to each other, differ materially from those of Mr. Hatchett. It is not indeed without a degree of diffidence that I dissent from so distinguished a chemist; but I conceive that I am justified by the experiments related in this essay.— Mr. Hatchett, in the valuable paper to which I have already referred, speaks of the white of the egg as consisting of pure albumen; but I believe that in this particular he will be found not perfectly accurate.

Animal mucus resembles veget. gum.

There is a great resemblance between the mechanical properties of animal mucus and vegetable gum, and I found that they strongly resemble each other also in their chemical qualities. A solution of gum arabic, containing one grain of gum to 200 grains of water, was not affected either by the oxymuriate of mercury or by tan. With the nitro-muriate of tin and with the nitrate of silver there was only a slight degree of opacity, but with the aqua lithargyri acetati there was a dense precipitate instantly formed.

On

V.

On muscular Motion. By ANTHONY CARLISLE, *Esq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1804.

(Concluded from Page 201.)

THE loss of motion and sensation from the influence of low temperature, accompany each other, and the capillaries of the vascular system appear to become contracted by the loss of animal heat, as in the examples of numbness from cold. Whether the cessation of muscular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muscles themselves, is doubtful; but the known influence of cold upon the sensorial system, rather favours the supposition that a certain temperature is necessary for the transmission of nervous influence, as well as sensation. Cold destroys mobility and sensation.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was submersed in water at 48°, and firmly retained there; air-bubbles began instantly to ascend, and continued during four minutes; the animal was not yet anxious for its liberty. After seven minutes it began to look about, attempting to escape; at ten minutes it rolled itself up, only protruding the snout, which was hastily retracted on being touched with the finger, and even the approach of the finger caused it to retract. After fifteen minutes complete submersion, the animal still remained rolled up, and withdrew its nose on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmosphere of 62°, with its head inclined downwards; it soon began to relax the sphincter muscle which contracts the skin, slow respirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog submersed in water at 94°, remained quiet until after five minutes; about the eighth minute it stretched itself out, and expired at the tenth. It remained relaxed, and extended, after the cessation of the vital functions; and its muscles were relaxed, contrary to those of the animal drowned in the colder water. Hybernating animals not easily drowned. Experiment with the hedge-hog.

The irritability of the heart is inseparably connected with respiration. Whenever the inhaled gas differs in its properties from Connection of irritability with respiration, &c.

from the common atmoſphere, the muſcular and ſenſible parts of the ſyſtem exhibit the change: the actions of the heart are altered or ſuſpended, and the whole muſcular and ſenſorial ſyſtems partake of the diſorder: the temperature of animals, as before intimated, ſeems altogether dependant on the reſpiratory functions, although it ſtill remains uncertain in what manner this is effected.

Diſtribution of heat by the blood.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the ſame individual at various times, or in its ſeveral parts, are always connected with the degree of rapidity of the circulation. It is no very wide ſtretch of phyſiological deduction to infer, that this increaſed temperature is produced by the more frequent expoſure of the maſs of blood to the reſpiratory influence, and the ſhort time allowed in each circuit for the loſs of the acquired heat.

Coagulation of that fluid.

The blood of an animal is uſually coagulated immediately after death, and the muſcles are contracted; but, in ſome peculiar modes of death, neither the one, nor the other of theſe effects are produced: with ſuch exceptions, the two phenomena are concomitant.

Heat delays the laſt coagulation and the contractions of the muſcles, &c.

A preternatural increaſe of animal heat delays the coagulation of the blood, and the laſt contractions of the muſcles: theſe contractions gradually diſappear, before any changes from putrefaction are manifeſted; but the cup in the coagulum of blood does not relax in the ſame manner; hence it may be inferred, that the final contraction of muſcles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-veſſels, becauſe ſuch contractions are not general throughout thoſe ſubſtances. The coagulation of the blood is a certain criterion of death. The reiterated viſitations of blood are not eſſential to muſcular irritability, becauſe the limbs of animals, ſeparated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The chemical combinations of living matter are tranſient, and not renewable.

The conſtituent elementary materials of which the peculiar animal and vegetable ſubſtances conſiſt, are not ſeparable by any chemical proceſſes hitherto inſtituted, in ſuch manner as to allow of a recombination into their former ſtate. The compoſition of theſe ſubſtances appears to be naturally of tranſient duration, and the attractions of the elementary materials which form

form the gross substances, are so loose and unsettled, that they are all decomposed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extensive discussion of the chemical properties attaching to the matter of muscle would be a labour unsuited to this occasion; I should not, however, discharge my present duty, If I omitted to say, that all such investigations can only be profitable when effected by simple processes, and when made upon the raw materials of the animal fabric, such, perhaps, as the albumen of eggs, and the blood. But, until by synthetical experiments the peculiar substances of animals are composed from what are considered to be elementary materials, or the changes of organic secretion imitated by art, it cannot be hoped that any determinate knowledge should be established upon which the physiology of muscles may be explained. Such researches and investigatious promise, however, the most probable ultimate success, since the phenomena are nearest allied to those of chemistry, and since all other hypotheses have, in their turns, proved unsatisfactory.

Difficulties attending chemical research into these objects.

Facts and Experiments tending to support and illustrate the preceding Argument.

Temperature of the primary fluids in different animals. The horse.

An emaciated horse was killed by dividing the medulla spinalis, and the large blood-vessels under the first bone of the sternum.

The temperature of the flowing blood was	103°
Spleen - -	103
Stomach - -	101
Colon - -	98
Bladder of urine	97
Atmosphere -	30.

Pigs.

Three pigs, killed by a blow on the head, and by the immediate division of the large arteries and veins, entering the middle of the basis of the heart, had the blood flowing from these vessels of 106, 106½, and 107°; the atmospheric temperature being at 31°.

An ox.

An ox, killed in a similar manner, the blood 103; atmosphere 50°.

Sheep.

Three sheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmosphere 41°.

Frogs.

Three frogs, kept for many days in an equable atmofphere at 54°; their ftomachs 62.°

Fluid of dropfy.

The watery fluid iffuing from a perfon tapped for dropfy of the belly 101°: the atmofphere being 43°, and the temperature of the fuperficies of the body at 96°.

Thefe temperatures are confiderably higher than the common eftimation.

Experiment to fhew that the volumes of mufcles are increafed during action.

A man's arm being introduced within a glafs cylinder, it was duly clofed at the end which embraced the head of the humerus; the veffel being inverted, water at 97° was poured in, fo as to fill it. A ground brafs plate clofed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, fo that the barometer tube became a gage, and no air was fuffered to remain in the apparatus. On the flighteft action with the mufcles of the hand, or fore-arm, the water afcended rapidly in the gage, making librations of fix and eight inches length in the barometer tube, on each contraction and relaxation of the mufcles.

Crimping of fifh cannot be effected after rigidity of death.

The remarkable effects of crimping fifh by immerfion in water, after the ufual figns of life have difappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this procefs may be practifed with fuccefs. The fea fifh deftined for crimping are ufually ftruck on the head when caught, which, it is faid, protracts the term of this capability; and the mufcles which retain this property longeft are thofe about the head. Many tranverfe fections of the mufcles being made, and the fifh immerfed in cold water, the contractions called crimping take place in about five minutes; but, if the mafs be large, it often requires thirty minutes to complete the procefs.

Experiment. Two fifh were fcored and one of them crimped. Its fpecific gravity, and alfo its abfolute weight, was increafed by crimping.

Two flounders, each weighing 1926 grains, the one being in a ftate for crimping, the other dead and rigid, were put into water at 48°, each being equally fcored with a knife. After half an hour, the crimped fifh had gained in weight 53 grains, but the dead fifh had loft 7 grains. The fpecific gravity of the crimped fifh was greater than that of the dead fifh, but a quantity of air-bubbles adhered to the furfaces of the crimped mufcles, which were rubbed off before weighing; this gas was not inflammable.

The

The specific gravity of the crimped fish - - 1,105
of the dead fish, after an equal
immersion in water - 1,090.

So that the accession of water specifically lighter than the muscle of fish, did not diminish the specific gravity of crimped muscle, but the contrary: a proof that condensation had taken place.

A piece of cod-fish weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another less vivacious piece, of fifteen pounds, gained one ounce and half*.

Other substances crimped with like effect.

The hinder limb of a frog, having the skin stripped off, and weighing $77\frac{1}{10}$ grains, was immersed in water at 54°, and suffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The specific gravity of the contracted limb had increased, as in the crimped fish.

Six hundred and thirty grains weight of the subscapularis muscle of a calf, which had been killed two days from the 10th of January, was immersed in New River water at 45°. After ninety minutes, the muscle was contracted, and weighed in air 770 grains: it had also increased in specific gravity, but the quantity of air-bubbles formed in the intersticial spaces of the reticular membrane made it difficult to ascertain the degree.

Some of the smallest fasciculi of muscular fibres from the same veal, which had not been immersed in water, were placed on a glass plate, in the field of a powerful microscope, and a drop of water thrown over them, at the temperature of 54°, the atmosphere in the room being 57°. They instantly began to contract, and became tortuous.

Fibrils of muscles contract by application of water.

On confining the ends of another fibril with little weights of glass, it contracted two-thirds of its former length, by similar treatment. The same experiment was made on the muscular fibres of lamb and beef, twelve hours after the animals had been killed, with the like results. Neither

* I am informed that the crimping of fresh water fishes requires hard water, or such as does not suit the purposes of washing with soap. This fact is substantiated by the practice of the London fishmongers, whose experience has taught them to employ pump water, or what is commonly called hard water.

 vinegar,

but not by acid or faline water. vinegar, nor water faturated with muriate of foda, nor ftrong ardent fpirit, nor olive oil, had any fuch effect upon the mufcular fibres.

Cold renders mufcles torpid. The amphibia, and coleopterous infects, become torpid at 34°. At 36° they move flowly, and with difficulty; and, at a lower temperature their mufcles ceafe to be irritable. The mufcles of warm-blooded animals are fimilarly affected by cold.

Mufcles of frogs irritable after freezing. The hinder limbs of a frog were fkinned and expofed to cold at 30°, and the mufcles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The fame procefs was employed in the temperature of 20°. and the mufcles kept frozen for twelve hours, but that did not deftroy the irritability.

Heat deprives mufcles of their irritability; In the heat of 100°, the mufcles of cold-blooded animals fall into the contractions of death; and at 110°, all thofe of warm blood, as far as thefe experiments have been extended. The mufcles of warm-blooded animals, which always contain more red particles in their fubftance than thofe of cold blood, are foon deprived of their irritability, even although their relative temperatures are preferved; and refpiration in the former tribe is more effential to life than in the latter.

as do poifons, Many fubftances accelerate the ceffation of irritability in mufcles when applied to their naked fibrils, fuch as all the narcotic vegetable poifons, muriate of foda, and the bile of animals; but they do not produce any other apparent change

and electricity. in mufcles, than that of the laft contraction. Difcharges of electricity paffed through mufcles, deftroy their irritability, but leave them apparently inflated with fmall bubbles of gas; perhaps fome combination obtains which decompofes the water.

Experiments on mufcles immerfed in fluids. The four feparated limbs of a recent frog were fkinned, and immerfed in different fluids; *viz.* No. 1, In a phial containing fix ounces by meafure of a faturated aqueous folution of liver of fulphur made with potafh; No. 2, In a diluted acetic acid, confifting of one drachm of concentrated acid to fix of water; No 3, in a diluted alkali, compofed of cauftic vegetable alkali one drachm, of water fix ounces; No. 4, in pure diftilled water.

The

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muscles were highly irritable.

The limb in No. 2, after the same duration, had become rigid, white, and swollen; it was not at all irritable. By removing the limb into a diluted solution of vegetable alkali, the muscles were relaxed, but no signs of irritability returned.

No. 3, under all the former circumstances, retained its previous appearances, and was irritable, but less so than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Muscular irritability destroyed by other causes.

Other causes of the loss of muscular irritability occur in pathological testimonies, some examples of which may not be ineligible for the present subject. Workmen whose hands are unavoidably exposed to the contact of white lead, are liable to what is called a palsy in the hands and wrists, from a torpidity of the muscles of the fore arm. This affection seems to be decidedly local, because, in many instances, neither the brain, nor the other members, partake of the disorder; and it oftenest affects the right hand. An ingenious practical chemist in London has frequently experienced spasms and rigidity in the muscles of his fore arms, from affusions of nitric acid over the cuticle of the hand and arm. The use of mercury occasionally brings on a similar rigidity in the masseter muscles.

Less blood flows through a contracted muscle.

A smaller quantity of blood flows through a muscle during the state of contraction, than during the quiescent state, as is evinced by the pale colour of red muscles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæsection, when the muscles of the limb are kept rigid, and the increased flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muscular fibrils takes place during each contraction, and that its free course obtains again during the relaxation. This state of the vascular system in a contracted muscle, does not, however, explain the diminution of its bulk, although it may have some influence on the limb of a living animal.

When

A contracted muscle is less sensible.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed; this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts with impunity: it is indead reasonable to expect, *a priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time *.

Moral causes influence the muscles in the human species.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations, are known to exhaust the muscular strength: intense thought and anxiety, weaken the muscular powers, and the passions of grief and fear produce the same effect suddenly: whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

Mental as well as muscular actions may by habit become automatic,

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic, and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which, the actions become automatic, demand little attention, cease to be irksome, and are effected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

and then give little fatigue.

Voluntary and involuntary muscles.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary; and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent; nevertheless the exigencies of the animal

* I have often observed that a small electric shock may be received without pain through the muscles of the fore arm; but I imagined it to be owing to the want of power in such a shock to increase the contraction.—N.

animal feelings eventually urge the respiratory movements in despight of the will. These last muscles appear to have become automatic by the continuance of habit.

Voluntary actions require education; the involuntary are caused by stimuli.

The uses of voluntary muscles are attained by experience, imitation, and instruction: but some of them are never called into action among Europeans, as the muscles of the external ears, and generally the occipito-frontalis. The purely involuntary muscles are each acted upon by different substances, which appear to be their peculiar stimuli; and these stimuli co-operate with the sensorial influence in producing their contractions: for example, the bile appears to be the appropriate stimulus of the muscular fibres of the alimentary canal below the stomach, because the absence of it renders those passages torpid. The digested aliment, or perhaps the gastric juice in a certain state, excites the stomach. The blood stimulates the heart, light the iris of the eye, and mechanical pressure seems to excite the muscles of the œsophagus. The last cause may perhaps be illustrated by the instances of compression upon the voluntary muscles, when partially contracted, of which there are many familiar examples. Probably the muscles of the ossicula auditûs are awakened by the tremors of sound; and this may be the occasion of the peculiar arrangement observable in the chorda tympani, which serves those muscles.

Stimuli seem to excite sensorial power in some respect resembling the passions.

These extraneous stimuli seem only to act in conjunction with the sensorial power, derived by those muscles from the gangliated nerves, because the passions of the mind alter the muscular actions of the heart, the alimentary canal, the respiratory muscles, and the iris; so that probably the respective stimuli already enumerated, only act subserviently, by awakening the attention of the sensorial power, (if that expression may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of sympathetic nerves, is effected without consciousness: for, when the attention of the mind, or the more interesting passions prevail, all the involuntary muscles act irregularly, and unsteadily, or wholly cease. The movements of the iris of the common parrot is a striking example of the mixed influence.

Lower tribes of animals act by external excitements.

The muscles of the lower tribes of animals, which are often entirely supplied by nerves coming from ganglions, appear of this class; and thus the animal motions are principally regulated

lated by the external ſtimuli, of which the occurrence ſeems to agree with the animal neceſſities: but the extenſive illuſtrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

The nervous power ſeems conſtantly active through life.

There are two ſtates of muſcles, one active, which is that of contraction, the other, a ſtate of ordinary tone, or relaxation, which may be conſidered paſſive, as far as it relates to the mind; but the ſenſorial or nervous power ſeems never to be quieſcent, as it reſpects either the voluntary or involuntary muſcles during life. The yielding of the ſphincters appears to depend on their being overpowered by antagoniſt muſcles, rather than on voluntary relaxation, as is commonly ſuppoſed.

I have now finiſhed this endeavour to exhibit the more recent hiſtorical facts connected with muſcular motion.

Concluſion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be propoſed. I have borrowed from the labours of others, without acknowledgement, becauſe it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myſelf, and I have adduced many general aſſumptions and concluſions, without offering the particular evidence for their confirmation, from a deſire to keep in view the remembrance of retroſpective accounts, and to combine them with intimations for future reſearch. The due cultivation of this intereſting purſuit cannot fail to elucidate many of the phenomena in queſtion, to remove premature and ill-founded phyſiological opinions, and eventually to aid in rendering the medical art more beneficial, by eſtabliſhing its doctrines on more extenſive and accurate views of the animal economy.

VI.

On the Meaſure of Mechanic Power. In a Letter from Mr. J. C. Hornblower.

To Mr. NICHOLSON.

Dear Sir,

Meaſure of mechanic power or effect.

I AM glad to find that ſomebody has ſeconded my motion concerning *horſe power*, and I hope the ſubject will not be diſmiſſed

mitted until it has passed the unanimous assent of both theoretic and practical mechanics; and I must here express my acknowledgments to Mr. Gregory for his improved method of deciding the question. It is of absolute importance, that the draught be in a circular direction, and also that the radius of the circle be given; for no position was ever more demonstrable than that the less the radius the less can a horse (whose sides are equal) exert his faculty of traction.

Measure of mechanic power, or effect.

But I must beg leave to use a little freedom (in no wise unbecoming, I hope,) in adverting to what Mr. Gregory in conjunction with Professor Robison has advanced on the subject of Mr. Smeaton's mode of defining *mechanic powers* and *mechanical effect*; and I am surprised that among men of talent and assiduity there can be a difference of opinion. It seems to me, that if any reason can be found, it must be that we do not understand the subject, and, perhaps I may give a decided proof of it in what I shall advance concerning it.

However, I am sure that when a ball of cast iron, of twenty inches diameter is elevated by means of a pinion and wheel connected to the sides of a pair of sheers, and left to hang there a little while,—if I cut the rope that sustains it, it will fall freely a certain height, in a certain portion of time, and would dash a faulty cylinder of a steam engine in pieces. And I am so well satisfied that there was a certain tendency in this ball to fall towards the centre of the earth, that I need not take a moment to examine the truth of it; but that the destruction of the cylinder was occasioned by the ball falling from the point to which it was raised, (how it was raised is no part of the subject.) Then I say, that if the ball had not fallen from the height it was, or if the ball had not been so heavy as it was, or if it had had its velocity retarded by any means, the cylinder would not have been broken. I will add, that had this ball been a true sphere, and the cylinder had not been there, but a certain curved surface in its place, which should have received the ball to prevent it impinging in the line of descent, it would have been turned out of its course conformably to the nature and position of the curve: and all that is above common apprehension in this matter, is, that while the ball was falling, its velocity was increasing every instant, and that when it met with the curve (if it gave it an horizontal course) it would proceed with uniform velocity just

Measure of mechanic power or effect.

just twice the space through which it fell, allowing the same time for this horizontal course as it had in falling its perpendicular height, and that its tendency is to continue that course for ever.

From this statement of the law of bodies in motion, (so far as it goes) I think we need not be very diffident in saying, that it is from similar facts, (though less philosophically observed) that we obtain our primary ideas of motion. The apple falling from the tree is a very good instance to the point, and it would not require a very extraordinary stretch of genius to apply such an accident as that to any thing like the pile engine, stamping press, &c.

But there are certain speculative mechanics, who in their mode of accounting for effects like what are here stated, have chosen to adopt terms of a very different import, and for some latent reason wish to keep gravitation out of sight. The writer of the article Dynamics in the Supplement of the Encyclopedia Britannica, calls it pressure, and by his way of philosophising in the explanation of the measure of mechanic power, has (in my opinion) laboured to make it as mysterious as possible.

I must for the sake of those of your readers who have not the work to refer to, quote a few passages now and then from the above popular work, and here I would refer them to the article on Machinery, where he begins by stating, that different notions have been entertained on this subject by Leibnitz, des Cartes, and other eminent mechanics of the last century, and adds, "*that some of the most eminent practitioners of the present times (for we must include Mr. Smeaton in the number) have given measures of mechanical power in machinery, which we think inaccurate, and tending to erroneous conclusions and maxims.*"

He then proceeds to explain and demonstrate the true measure of mechanic power, and he begins by supposing a man pressing uniformly on a mass of matter for a certain time, and going on with the subject takes occasion to distinguish between *the weight of a body* and *its heaviness!* and, towards the latter end of that section he comes to some sort of a conclusion of the subject, so far as to say what is the real measure of mechanic power: I see I must make endless quotations if I regard the very letter of his argument, but I hope I shall be excused; however I will quote the last paragraph verbatim.

Relating

Relating to the pressure of the man just mentioned, he says, "but farther we know, that this pressure is the exertion *, we have no other notion of our own force, and our notion of gravity, of elasticity, or any other natural force is the same. We also know, that the continuance of this exertion fatigues and exhausts our strength as completely as the most violent motion. A dead pull as it is called of a horse at a post fixed in the ground, is a usual trial of his strength. No man can hold out his arm horizontally for much more than a quarter of an hour, and the exertion of the last minutes gives the most distressing fatigue, and disables the shoulder for action for a considerable time after. *This is therefore an expenditure of mechanical power in the strict primitive sense of the word.* Of this expenditure we have an exact and adequate effect and measure in the quantity of motion produced, that is in the product of the quantity of matter by the velocity generated in it by this exertion. And it must be particularly noticed, that the measure is applicable even to cases where no motion is produced by the exertion; that is, if we know that the exertion which is just unable to start a block of stone lying on a smooth pavement, but would start it if increased by the smallest addition, and if we know that this would generate in a second 32 feet of velocity in 100 pounds of matter, we are certain that it was a pressure equal to the weight of this 100 pounds. It is a good measure, though not immediate, and may be used without danger of mistake when we have no other."

Measure of mechanic power or effect.

I should not have quoted so much of this section, if it had not been that I think it contains an unequivocal interpretation of the writers notion of the true measure of mechanic power, and at once exhibiting, in my proud opinion, the fallacy of the doctrine in *toto*. What! shall mere muscular exertion, whether of horse or man, be esteemed even an auxiliary to get the conception of the nature of the thing; how much less then shall it be set forth as the thing itself? In perfect consonance with this writer, Mr. Gregory, page 152, Philosophical Journal, Vol. XI. says, suppose that a horse while standing still, sustains by means of a rope and simple fixed pulley, a mass of an hundred weight, and thus keeps it suspended at the top of a well for the space of a minute; neither the animal nor the weight moves: but shall we say, in conformity

* Article Machinery, Supplement, 4th sec.

as

Measure of mechanic power or effect.

as it would seem with Mr. Smeaton's measure, that there is no power expended, no effect produced. On the contrary we know there is a power expended, and that effect, if sufficiently long continued, would completely tire the horse. Then let us have a *post* instead of the horse, and surely that will not tire, and what will be the consequence then? why then there will be no power expended, and no effect produced; and I beg leave to ask my opponents, what is the power expended when a horse or other animal is placed there to sustain the weight? is it any more than the expence of nervous or muscular action; and has that any analogy with a weight descending through a given space, either uniformly or accelerated? and, I ask again, what is the effect produced more than what is produced by the post? the horse does but keep the weight from dropping into the well, and the post will do the same; indeed you may say, that when you hang up your hat, that the pin which sustains it, prevents it from falling, as does the horse the mass in the well, and that therefore there must be some power expended on the pin.

It is really difficult to be grave on this occasion; but I feel myself restrained by the magnitude of the subject, and its importance to the community. Professor Robison says, when a man holds out his arm horizontally, the exertion towards the end of a quarter of an hour gives the most distressing fatigue, and then says this is an expenditure of mechanical power, which I shall take the liberty to deny for the present. But is it such a mechanical power as Smeaton's, or in fine, is it a power made up of a mass of matter moving with any determinate velocity either uniform or accelerated? If the learned Professor intended to familiarize the doctrine to people of common sense, he could not have chosen a more indirect and perplexing example.

But let us attend a little further to the subject in the fourth section of the article Machinery, Sup. Ency. Brit. There he says, that "when a man supports a weight for a single instant, he certainly balances the pressure or action of gravity on that body," by the way here is a great want of precision in the expression, "*pressure or action of gravity*," as if they were synonimous terms, whereas *pressure* certainly denotes *repulsion*, if the term will bear any definition at all, and to explain the term *gravity* if it will not admit of *attraction*, I am sure

fure it cannot be called repulfion, but to proceed, "and he continues this action as long as he continues to fupport it, and we know, that if this body were at the end of a horizontal arm turning round a vertical axis, *the fame effort* which the man exerted in merely carrying the weight, if now exerted on the body by pufhing it horizontally round the axis, will generate in it the fame velocity which gravity would generate by its falling freely."

Meafure of mechanic power or effect.

A more erroneous propofition was never introduced to the theory or practice of mechanics. What, is there no difference in a man carrying a load on his fhoulder, and putting it into a truck? or to come nearer the Profeffor's propofition, let the man who has to carry two hundred pounds for one mile be permitted to take the weight from his fhoulders, and reft it on the arm of any thing like a horfe wheel, perfectly detached from the mill gear, let the gudgeons be oiled, and then let him "*pufh*" it horizontally round its axis until he has travelled a mile.

Now without afking the man which he likes beft, let us fee what he does by placing his load on the arm of the horfe-wheel, and pufhing it round. Why, he certainly overcomes the additional friction which his load has added to the weight of the wheel, and that is all, and if you will let us have gudgeons which have no friction, the man need not to walk far to pufh the horizontal arm into perpetual motion.

But now for the monftrous conclufion by this propofition, "If the man's exertion was employed to generate motion inftead of counteracting gravity, he would generate during that minute the fame motion that gravity would; that is 60×32 feet velocity per fecond in a mafs of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per fecond. We would exprefs this production or effect by 30×1920, or by 57600 as the meafure of the man's exertion during the minute."

Here is evidently a typographical error, *fecond* for *minute*, but when we admit fuppofitions for the fake of illuftration, there ought to be fome conformity in the fuppofition to the fact it is intended to illuftrate: then I would afk, where's the man who can generate 32 feet of velocity in a mafs of 30 pounds in one fecond? to be fure he can let it fall a fecond, but he cannot *carry* it 32 feet in a fecond; but he fays, "we

4 would

Measure of mechanic power or effect.

would express the production or effect by 30 × 1920 or 5760 as the measure of the man's exertion during the minute." Sir, it is more than even one of Boulton and Watts' horses can do.—Well may he say, "such an exertion will completely exhaust a man's strength."

He then goes on to consider "*more narrowly* what a man *really does*, when he performs what Mr. Smeaton allows to be the production of a measurable mechanical effect. Suppose a weight of 30 pounds hanging by a cord which passes over a pulley, and that a man taking this cord over his shoulder, turns his back to the pulley, and walks away from it, we know that a man of ordinary force will walk along raising this weight at the rate of about sixty yards in a minute, or a yard in every second, and that he can continue to do this for eight or ten hours from day to day, and that this is all he can do without fatigue. Here are 30 pounds raised uniformly 180 feet in a minute, and Mr. Smeaton would express this by 30 × 180 or 5400, and would call this the measure of the mechanical effect, and also of the expenditure of power. This is very different from our measure 57600."—Yes, but I hope not the less conclusive on that account.

It is wholly incomprehensible to me why those men (who have certainly a right to controvert any proposition which appears to them erroneous) should take up the subject, assuming points which the doctrine advanced by Mr. Smeaton has nothing to do with. It is clear that all this animal exertion comes at last to the law of bodies falling in accelerated velocity, which Smeaton allows to be a distinct consideration, as he says, "if the weight descends quickly, it is sensibly compounded with another law, viz. the law of acceleration by gravity." But how inconsistent is it to go about to elucidate the laws of bodies in motion by the action of a man or a horse. What is the expenditure of animal power but a waste of what has been usually termed by anatomists nervous spirits, or perhaps an inceptive disorganization of the constituent parts of the ligaments and muscles? in short, we may compare it to contractions, inflammation, and gangrene, but never to the momentum acquired by a body moving through a certain space in a certain time. It is remarkable, that all this reasoning is about that of which Mr. Smeaton has never said a word, except in his illustration of the mechanic power

necessary

neceffary to give velocity to heavy bodies, where he fuppofes a man pufhing an iron ball on an extended plane, and this not to try his mufcular force, or to fee what he can do without tiring, but merely in elucidation of the doctrine he fets out with.

Meafure of mechanic power or effect.

I have not time at prefent to purfue the fubject to the extent I wifh, and to enter on the ground of the miftake of this great man: nor let it be imagined for a moment that I have availed myfelf of his everlafting abfence, to call in queftion what he has advanced in refutation of a fuppofed error. It would have remained as it was unto a diftant period, had it not been, that I fee Mr. Gregory advancing the fame opinion, which no doubt he will defend or defert.

I am, Dear Sir,

Your obedient Servant,

J. C. HORNBLOWER.

If Mr. Gregory fhould read this, I fhall be glad if he will fet me right as to the identity of *animal exertion* and *mechanic power.*

VII.

An Inveftigation of all the Changes of the variable Star in Sobiefki's Shield, from five Years Obfervations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures refpecting unenlightened heavenly Bodies. By EDWARD PIGOTT, *Efq. Abridged from the Philofophical Tranfactions for* 1805.

THE author begins his memoir with an inveftigation of the periods of change in the variable ftar in Sobiefki's fhield, of which the right afcenfion was 279° 9½′, and its declination fouth 5° 56′, for end of June 1796. Its rotation on its axis in 1796 was eftimated at 62¾ days, from a mean of fix obfervations of its greateft and leaft brightnefs. In the prefent paper he gives about 26 fimilar determinations, moft of them the refults of very accurate obfervations made in the year 1796, 1797, 1798, 1799, and 1801. From all thefe refults it

The variable ftar in Sobiefki's fhield, revolves on its axis in 61½ days.

it was found that the disagreements between the periods of change, as deduced from its full brightness, were much greater than those deduced from its least illumination. The former gave the mean period 63 days, and the latter $59\frac{3}{4}$; and the mean of these, namely $61\frac{1}{2}$ days, agreeing with the former determination to $\frac{1}{4}$ day, is as near as could be expected in observations of this nature.

The places of full and least brightness do not equally divide the star's circumference; they divide it as 7 to 8.

The author in the next place proceeds to examine some other of the changes to which this star is subject. By tabulated observations through the same series of years, he finds that the time of decrease, from the middle of its full brightness to the middle of its least, is on a mean of 34 days; and that the time of its increase, from the middle of its least brightness to the middle of its full, is in like manner only 27 days. The sum of these numbers amounting to the period 61, shews their probable exactness. These compared and combined with the former determinations of 1796, give a mean of the whole 33 + and 29 – days. As it thus appears that the time of the decrease is longer than that of the increase, it follows of course, that the places of the full and least brightness are not situated at the distance of half the circumference from each other; and the like circumstance Mr. P. affirms is found to be the case with most, if not all the variable stars.

Other variable stars are similarly affected.

The luminous parts are themselves variable.

The next particulars to be reviewed were the durations of its brightness without any perceptible change, while at its maximum and minimum. These determinations required a tolerable succession of observations; where that is not the case he has omitted them in his tables. From these it is found in general, that when the degree of brightness at its maximum is less than usual, and its minimum not much decreased, the changes take place but very slowly, and cannot be settled with much accuracy unless the observations have been made frequently and with great attention. He accordingly passes again over the series of years, shewing the dates of its magnitudes when at its full brightness, and also when at its least brightness, and he tabulates at the end of the first part of his paper, all the different changes that have been examined. The words in the first column or compartment describe them; in the second column the present results are exhibited; in the third are the results of the former observations; and in the last column is placed a mean of both computed proportionally, according to the number of observations of each.

TABLE

TABLE VIII.

Table of the general affections of the variable star.

	Days.	Days.	Days, on a mean.
Rotation on its axis - - -	61½	62¼	62—
Duration of brightneſs, at its maximum, without any perceptible change - - - - -	8+	14	9½
Ditto, when it does not attain its uſual brightneſs - - -	20—	—	—
Duration of brightneſs at its minimum, without any perceptible change - - - - -	9—	9	9
Ditto, when it does not decreaſe ſo much as uſual - - -	20—	—	—
Decreaſe in time, from the middle of its full brightneſs to the middle of its leaſt - - - - -	34	28	33+
Increaſe in time, from the middle of its leaſt brightneſs to the middle of its full - - -	27+	35	29—
Extremes of its different degrees of brightneſs; with a mean of its uſual variations -	5+ 9 or 0	5+ 7.8	5. 6

The changes in the luminous parts occaſion the periods ſingly taken to differ from each other.

The author having thus ſettled with conſiderable preciſion theſe eſſential variations of the ſtar, proceeds to examine ſome of its other phenomena, particularly one which is common to moſt of the changeable ſtars, and likewiſe in ſome degree to our ſun, namely, that the times of their periodical returns of brightneſs are in general irregular,—a circumſtance ſo intereſting as to engage our attention, and which induced Mr. P. to make the ſucceſſion of obſervations, in the hope of diſcovering its nature and cauſe. With this view he has proceeded to tabulate the ſeries of years, ſo as to aſcertain the apparent rotations in days from the obſerved middle times of its full brightneſs, and alſo from the obſerved middle times of its leaſt brightneſs, for ſingle periods. From theſe it appears, that the periodical returns of brightneſs are uncommonly fluctuating, and that the differences between the extremes are very conſiderable; to account for which he offers the following

ing explanations, ſuggeſting previouſly a few plauſible conjectures, and ſome inferences ariſing from the obſervations themſelves *.

Aſſumed poſitions. 1. The ſtars are opake. 2. They have regular rotations. 3. Their luminous appearance is cauſed by an atmoſphere like that of the ſun. 4. The luminous parts are ſparingly diſperſed in the ſtar here treated of:

1ſt. That the body of the ſtars are dark and ſolid.

2d. Their real rotations on their axes are regular.

3d. That the ſurrounding medium is by times generating and abſorbing its luminous particles in a manner nearly ſimilar to what has been lately ſo ingeniouſly illuſtrated by the great inveſtigator of the heavens, Dr. Herſchel, with regard to the ſun's atmoſphere.

4th. That theſe luminous particles are but *ſparingly diſperſed* in the atmoſphere ſurrounding the variable ſtar of Sobieſki, appears from the ſtar being occaſionally diminiſhed to the 6.7 magnitude, and much leſs. July 4, 1799, it was of the 7th; September 15, 1798, and Auguſt 9, 1803, *of the 9th, if not inviſible.* (See Table VII †.) Does not this indicate a very ſmall portion of light on its *darkened hemiſphere?*

5. Probably ſmall patches:

5th. And may we not with much plauſibility conſider them as ſpots, ſomewhat circular, or of no great extent? for even on its *brighteſt hemiſphere* the *duration* of its full luſtre is, on a mean, only 9½ days of the 62, or about one-ſixth and ½ of its circumference. (See Table VIII. page 140.) The dimenſions therefore of the parts enlightened ſeem much circumſcribed, and can be tolerably eſtimated, and conſequently may be repreſented very ſmall, particularly if the *powerful effect of a little light* and *the length of time* a bright ſpot is remaining in view be taken into conſideration.

6. Changeable in their nature:

6th. And a further ground of preſumption that thoſe principal bright parts are but ſlight patches is, that they undergo *perpetual changes,* and alſo that ſuch changes are very viſible to us, for moſt probably they would be imperceptible, were not the bright parts contraſted by conſiderable intervals or diminutions of light.

7. And deducible from the phenomena.

7th, and laſt. We may obtain ſome idea of the *relative ſituation or intervals between* theſe bright parts, by the obſerva-

* The reſt of the paper is given without abridgement, and the author himſelf ſpeaks in the firſt perſon.

† The author refers to his tables in the Tranſactions, of which the abridged reſult has been here given.

tions

tions of the increase and decrease of brightness, as thereby the changes and times elapsed are pointed out. (See Table V. page 136; and Phil. Trans. for 1797.)

Experiment with a sphere.

I have tried practically the effect of the above suppositions, by placing small white spots on a dark sphere, which being revolved round represented the various changes as nearly as could be expected: proceeding therefore with these and other considerations, I shall make ideal drawings of the star with the small illuminated parts in its atmosphere, and apply to them some of the actual observations from both the preceding tables, having always in view that each period may, more or less, require a different disposition of spots, in consequence of their constant changeability.

1st View.

View of the star at its greatest brightness.

Plate XIII. Fig. 1, A B, the star's polar axis, round which its rotation takes place in 62 days from C to D.

C D, its equator, the 360 degrees of which being revolved in 62 days, gives nearly $5\frac{3}{4}$ degrees for each day's motion; the brightest part or spot is represented as centrally facing us, and accordingly shewing the star in its greatest lustre. Were this bright spot and the other parts to remain *unchangeable*, they would after having completed the revolution of 360 degrees, or 62 days, (the star's rotation on its axis,) appear again as at first, and at every return continue to give exact periodical times, as was nearly the case in 1799, between August and October, (see Table IX. p. 142); but if the spot becomes obscure, and another brightens up in a different place, this latter will make the star appear at its next full splendour either sooner or later than the real rotation according to its position, thus,

2d View.

Anomaly from another bright spot causing an apparently short period.

Fig. 1. A full brightness having been shewn by the same spot, it afterwards loses its light, and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, see Fig. 2. This latter, when turned centrally to the earth, will appear 5 *days sooner* than the former one, now obscured, (here marked P,) and show the star at its full lustre, making the rotation 57 days instead of 62, which was the case in 1796, the observed revolution between September 17 and November 13. (See Table IX.)

3d View.

Another causing a period apparently long.

Fig. 3. We will now apply a case of an interval of too great length, that of 72 days: the spot *m* alone having shewn us the star in its full lustre, its light disappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the star will appear obscured, and not recover its splendour until the new brightened part H becomes central, which being *ten days later* than the position in which *m* was seen, makes the revolution 72 days instead of 62, as was observed between July 14 and September 24, 1801. (See Table IX.) In the above case the alterations took place while behind the star, otherwise some irregularities would have been perceived, as will later be noticed. The same reasoning with proper alterations will, I apprehend, account for the other revolutions, yet I shall soon again resume the subject with regard to a *series* of the greatest irregularities; at present let us proceed to take a few views of the intervals of its *least brightness*, which, contrary to my expectation, I find much more difficult to explain than those of the full, although the results disagree less among themselves. The darkened face of the star is here represented with a few small changeable bright spots, placed in general at a proper distance, so as to keep up an uninterrupted increase and decrease of light with regard to us, and are also made to correspond with several other observations.

4th View.

Greatest period explained; between intervals of least brightness.

Fig 4 is to explain the greatest interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemisphere here exhibited in its *minimum* July 4th, with the following spots, *w* nearly gone off, next a small one *l*, then another P of a similar size, preceding the centre a day or two, (or a few degrees, (and lastly a bright one at D, just appearing. During the rotation, D losing its light and the P becoming *much brighter*, the star at its next return in 62 days, when at its first position, must of course appear much brighter, (See fig. 5) but by the retiring of *l* and P continues to diminish in lustre till the appearance of some large spot from the other hemisphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution

revolution of 74 days, as required; for a view of a short interval, for the present let that of 56 days be taken between August 21st and October 16th 1801. (See Table X.)

5th View.

The least brightness or *minimum* is represented by fig. 6, when the bright spots *y* and *x* at each extremity of the equatorial diameter are mutually but just in sight and a minute one, *r* alone on its surface preceding *y* by 6 days motion: *n n*, are other middling sized spots near *x*, but preceding it; they cannot for the present be seen, being on the opposite or bright hemisphere. The spot *x* during the stars revolution having lost its light, and *r* being considerably increased, the next *minimum* will be between *n n* and *r*, (instead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the star's light will continue to take place only until the re-appearance of *r*, at the place where *y* was, which being 6 days sooner than the former position, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I shall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the most intricate irregularities and vicissitudes: these observations are already pointed at full length in the Philosophical Transactions for 1797, and therefore can at any time be inspected: indeed, I then little thought they would ever become of further use, but that of stating facts, to which, however, I have always been very partial, and particularly so, after having experienced the advantage of Maraldi's printed observations on the variable star in Hydra, as it was partly by them that I ascertained the periodical returns of brightness of that star, and which flattered me the more, as Maraldi himself had been less successful in the attempt; See Phil. Trans. for 1786. Yet in the present Paper I have omitted all such details, being aware they might be thought too voluminious, but hope at some future time the Society will honour them with a place in their library. Least brightness.

The first sketch, Plate XIV. represents, for June 13, 1796, the comparative size of the bright spots supposed to surround the star, but here extended at full length; the next eight following are

Explanation by the figures of the periodical variations and irregularities of light in the star in Sobieski's shield.

are spherical views, on an enlarged scale, for each quarterly, rotation or less, shewing the principal changes, as expressed in the adjoining remarks, and corresponding with the observations; these being taken from my printed paper, as already mentioned, are marked in italics. It will be seen that the spots by which the changes are principally regulated, are placed at equal distances, yet intermediate ones might also frequently be inserted without occasioning any objection, but that of rendering the explanations more complex.

REMARKS ON PLATE III.

Fig. 2. "*June 18th. Full brightness Mag. bright 5th,*" before or after which date the star would appear less bright, by the spot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July 3d,* 15 *days or* ¼ *rotation being elapsed since June* 18*th, 5th Mag, a little decreased*" by the removal of the brightest spot E, the *h* being much less.

Fig. 4. "*July* 19*th,* 16 *days or* ½ *rotation 5.6 Mag. still decreased,*" N being much less than *h*, now gone off. *A slight minimum.*"

Fig. 5. "*July 27th,* 8 *days of the rotation,* 5 *Mag. rather increased*" by the considerable increase of N since four days, with the addition of F, *a slight full brightness.*

Fig. 6. "*Aug. 3d,* 7 *days of rotation, 5,6 Mag. decreased* by the going off of N, the E, which is now reappearing, being reduced to much less than F.

Fig. 7. "*Aug.* 19*th,* 16 *days or* ½ *rotation,* 5.6 *Mag. again decreased,*" by the removal of F, by E being much less, and by the *h* also being considerably diminished.

Fig. 8. "*Sept 3d,* 15 *days or* ½ *rotation,* 6 *Mag. still more decreased,*" by the *h* being much less than E, which is now going off, and N scarcely reappearing, *another minimum.*

Fig. 9. "*Sept.* 17*th,* 14 *days or near* ½ *rotation,* 5 *Mag. full brightness considerably increased,*" by N having retained its increased brightness of July 27, and now facing us centrally.

1st, Thus are exhibited, the two short intervals of its full brightness, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly.

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

3dly, The long decreafe of 38 days between July 27 and Sept. 3, and

4thly, The rapid increafes of 8 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

Explanation by the figures of the periodical variations and irregularities of light in the ftar in Sobiefki's fhield.

As alfo the other intermediate changes, yet I muft again repeat, particularly as a few days error may occafionally proceed from the obfervations, that by thefe fketches it is not meant to give exact drawings of the fize, diftances or alterations of the fpots, but merely to fhew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating obfervations; nor do I prefume to think, that the explanations are the only ones or beft can that be imagined, the more fo, as they folely refer (for greater fimplicity) to the ftar's equator, while poffibly, were the fpots placed in a northern or fouthern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more fatisfactory: however, the materials themfelves, the *obfervations* and *deductions* will I flatter myfelf ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to fimilar parts of the ftarry fyftem, with regard to the probability of eftablifhing whether any of the moft *irregular or particular* changes may not *return at fixt periods*, or after a certain number of rotations. I think we can entertain but flight hopes of it, owing to the *great fluctuation* of the luminous matter, as fhewn by the *perpetual varying* of the *apparent* revolutions, magnitudes, &c. See Tab. IX. X. and VII. Still it is natural to fuppofe, that fome parts of the atmofphere of this ftar may have a lefs tendency than others to become luminous, fo as to promote at different times, fimilar appearances; and indeed that is ftrongly indicated by the *intervals* of the *minima* being far *more regular* than thofe of the *full brightnefs*, which, with other reafons induce us to fufpect even that one of its hemifpheres is lefs favourably conftituted or qualified, than the other for the generating of thefe particles, although they do occafionally encroach on both fides, as appears by the obfervations between June and Auguft, See Phil. Trans. for 1797, or the eight fketches of 1796, and likewife in 1797, fee Tab. VII. when during *three months* it was only reduced to the

5 or

5 or 6 Mag. by which the degree of brightnefs that furrounded it, muft have been nearly equal: had the caufes of varying its light then ceafed, it would ever have continued to appear as an unchangeable ftar of the 5 or 6 Mag. and fuch is the cafe of feveral others that *formerly have been variables*, but for many years retain a fteady brightnefs, as β Geminorum, δ Urfæ majoris, α Draconis, and perhaps that in the Swan's breaft, while others, after *fhewing their changes*, have *entirely difappeared*, owing to a total abforption of light, as the famous one in Caffiopea, in Serpentarius of 1604, that near the Swan's head, and doubtlefs many more. Does not this induce us to prefume that there are alfo others, that have *never fhewn* a glimpfe of brightnefs? Laftly, *new variables* may become fo at different periods, by an unufual and partial increafe or diminution of their bright parts, as not unlikely was the cafe of o Ceti, Algol α Herculis, &c. for thefe ftars being by times very confpicuous, their changes, had they been always equally great, might have been eafily noticed by the ancient aftronomers, who obferved only with the *naked eye*. A few lines above, I mentioned the probability that there exifted *primary* invifible bodies or *unenlightened ftars* (if I may be allowed the expreffion) that have ever remained in *eternal darknefs*; how numerous thefe may be, can never be known. Would it then be too daring or vifionary to fuppofe their numbers equal to thofe endowed with light? particularly when we take into contemplation the ample fet of bodies vifible only by reflected rays, that appertain to our own fyftem, fuch as the planets, afteroides, comets, and fatellites. Do not thefe, although but of a fecondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to fufpect, that the *enlightened ftars* are thofe that have already attained the higheft degree of perfection? granting, therefore, fuch multitudes do really exift, clufters of them, by being collected together as in the milky-way, muft intercept all more diftant rays, and if free from any intervening lights, they would appear as *dark fpaces* in the heavens, fimilar to what has been obferved in the Southern Hemifphere. That fo few of thefe obfcure places are perceived, may be attributed to their being obliterated by the prefence either of fome fcattered ftars, or of other flight luminous appearances.

Changeable ftars may become permanent; and others difappear. There may be dark ftars, perhaps equal in number to the vifible ones.

I have thus fully inveftigated the nature of this diftant fun, a fingle one among many millions, and fcarcely perceptible

Our fun, though variable, is ftill very luminous;

5 to

to the sight, yet of no less importance than our own grand luminary. But ours is still supplied abundantly with resplendent particles, while Sobieski's variable star has them most sparingly dispersed over its sphere: a scantiness that apparently must occasion to its surrounding planets, constant vicissitudes of uncertain darkness, and repletion of light and heat. How far more enviable seems our situation; I mean that which we enjoy at present; there being strong reasons to believe, that the sun's luminous appearance has been at times considerably diminished; and I have little hesitation in conceiving that it may also be reduced at some future period to small patches, and then the apparent irregularities of its periodical rotations, which at present are only perceived by the observations of trifling dark spots, would become evidently conspicuous, particularly when seen at a distance as remote as the variable stars are from us. But such conjectural flights of fancy cannot too soon be dropt. I therefore shall conclude with observing, that these inquiries on the alterations of light of the stars have been so little discussed, that it is to be hoped they will not be discontinued; and although I have already troubled the Society with many papers concerning such changes, I nevertheless propose, ere long, having the honour of presenting them with one more, most probably my last, on this subject.

though it may become reduced in future ages.

EDW. PIGOTT.

VIII.

Account of a Luminous Meteor. By a CONSTANT READER.

To Mr. NICHOLSON.

SIR,

HAVING found it frequently stated, that it might be useful, should every one who has a fair opportunity of noting with reasonable accuracy, the course and altitude of meteors, describe their appearances as well as they are able; I send you the following account to make what use of you please.

Description of the appearance, course, and duration of a luminous meteor.

Last night (Sunday the 21st) passing along the Strand, I stopped at the door of the Crown and Anchor, the vacant space before it lately caused by the pulling down of houses offering

Description of the appearance, course, and duration of a luminous meteor.

offering a considerable view of the heavens, at that time splendid with stars; I was looking with attention towards the N. W. when suddenly a meteor from about 35° of height, shot from the W. by N. It was apparently about the size of a tennis ball, perhaps hardly so large, it was followed by a stream of light which seemed in specks, the length of the train was about a degree, that is about twice the apparent diameter of the moon. Its course was from North of West towards the North, passing about 10° below the of the Great Bear, which I judge was then about 45° above the horizon. Its motion was majestic, by no means rapid, I am sure it was full ten seconds in motion, the light not so piercing as that of a star of the first magnitude, but exceeded that of the second, with which I had full opportunity of comparing it. It ran through 30° of the heavens, describing an arch of great diameter, its path was convex above, and declining downwards. The extinction of it was at an altitude of about 25° having fallen certainly not more than 10°, I do not think so much. It very visibly stopped before it was extinguished. It burst at last with very few sparks, and its train and itself together disappeared in a moment. I had perfect leisure and space to observe its whole course, it expired below the second pointer, I instantly drew out my watch, and comparing it this morning with the clock of St. Paul's Cathedral, it was exactly at thirty-one minutes after eleven that I observed the end of the phenomenon.

I am, Sir,

Your most obedient humble Servant,

A CONSTANT READER.

Monday, July 22d, 1805.

IX.

Precipitation of Platina as a Covering or Defence to polished Steel, and also to Brass. In a Letter from Mr. J. STODART.

To Mr. NICHOLSON.

DEAR SIR,

Platina is taken from its solution by ether.

YOU kindly favoured me, by inserting in the last Number of your excellent Journal, an account of a method I have

3 used

used with success, for gilding polished steel with gold; perhaps it may be worth knowing, that a very similar process may be performed with platina. That metal, in a state of solution, is taken up from the acid by agitation with ether, in the way that gold is, though certainly with less avidity. The ethereal solution of platina afforded by this process, is, like that of gold, deposited on the surface of polished iron, or steel, forming a coat of defence from rust. It is perhaps a fact of equal importance, that the surface of polished brass is coated with platina by the same operation that steel is; namely, by plunging the brass for an instant into the ethereal solution. As far as I know, these facts have not hitherto been noticed: on the contrary, authors highly respectable, have from ingenious and well conducted experiments, been led to conclusions very opposite to those I have advanced. Dr. Lewis, to whose genius and industry the arts are much indebted, says, "gold is the only one of the known metals which the ether takes from acids; and hence this fluid affords a ready method of distinguishing gold, contained in acid solutions." The same author gives the following experiment. "Sulphuric ether was poured into a solution of platina, and into a composition of platina and gold. The vials were stopt and shaken, the ether received no colour from the solution of platina, but became instantly yellow from that of the platina and gold." The only way in which I can account for these results, so contrary to my experience, is by supposing that the platina with which Dr. Lewis made his experiments, was not so pure as that with which we are now furnished. What I used was part of an excellent malleable bar, its specific gravity I do not exactly know. I am inclined to think it was quite pure. The ether was furnished by my friend Mr. Hume, whom I am again happy to thank for his kind and able assistance. The ethereal solution of platina is of a beautiful pale yellow colour, does not at all stain the hand, and is precipitated by volatile alcali. The precipitate I have not examined. It may be fulminating, and I have no relish for explosions. The coat of platina on steel is of a dull white colour. I have no doubt of its proving quite as good a defence from rust, as the coat of gold. It is, however, by no means so beautiful; for which reason a preference will probably be given to the last named metal. I have used both the gold and platina in coating different

and coats iron or steel;

and also brass.

Dr. Lewis's experiments gave a contrary effect;

probably because his platina was impure.

The platina coating is less beautiful than gold.

different parts of the ſame inſtrument. The effect produced by the contraſt of colour is very beautiful. Whether any of theſe obſervations may be worth communicating through the medium of your moſt uſeful Journal, is a queſtion I beg leave to ſubmit entirely to your judgment. I have not tried any of the eſſential oils with ſolution of platina; further experiments will probably be made with theſe metallic ſolutions, by thoſe who have more time, and a better knowledge of theſe ſubjects. Such purſuits, when the reſults are frankly communicated, promiſe to benefit ſcience, and muſt ultimately prove uſeful to ſociety.

I remain with much reſpect,

Dear Sir, your obliged Servant,

J. STODART.

Strand, July 24, 1805.

X.

Experiments on Wootz. By Mr. DAVID MUSHETT. *From the Philoſophical Tranſactions,* 1805.

(Concluded from Page 204.)

Forging No. 2.

Appearances on forging the cake of wootz, No. 2.

ONE half of this cake was heated to a ſcarlet ſhade, and put under the cutting chiſſel; it was at firſt ſtruck lightly, then reheated, and cut comparatively ſoft; but a ſmall crack had over-run the progreſs of the chiſſel. Its ſoftneſs in cutting was attributed to an evident want of ſolidity. The other half cake felt harder under the hammer, but proved afterwards ſpongy throughout the maſs. In the act of cutting, a looſe pulveriſed matter was diſengaged from ſome of the cells, poſſeſſed of a ſhining appearance.

The fractures obtained in conſequence of the diviſion of the half cakes, preſented a flattiſh cryſtallized appearance, more reſembling very white caſt iron, than ſteel capable of being extended under the hammer. One of the middle cuts was entirely cellular with cryſtallized interiors, and incapable of drawing; the correſponding cut of the other half cake was drawn

drawn into a ſtrait bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one ſhade lighter than No. 1, it tore leſs out, was equally yolky, and poſſeſſed on the whole an aſpect very unfavourable for good ſteel.

The other two outſide quarters were alſo drawn into ſhape, one under the tilt hammer, and the other by hand. Theſe were more ſolid in the fracture, poſſeſſed fewer ſurface-cracks, ſtood a higher degree of heat, tore out more, and exhibited a ſilky gloſſy grain, at leaſt two ſhades lighter in the colour than the centre pieces.

Forging 3d Cake.

Appearances on forging the cake of wootz, No. 3.

One half of this cake, firſt ſubjected to be cut, was found ſofter than any of the preceding, and exhibited no ſymptom of cracking. The other half was cut at three heats, but found looſe and hollow in the extreme. A conſiderable portion of the ſame brilliant powder, formerly noticed, was here again diſengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulvereſcent ſtate, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumſtance led me to examine every pore and cell throughout the whole fragments. On the upper ſurface of two of them I found ſmall pits containing a portion of the ore, which had been ſlightly agglutinated in the fire, but ſtill highly magnetic. The upper ſurface of the preſent cake, cloſe by the gate or feeder, contained a large pit filled with a ſtratum of ſemi-fuſed ore, ſurmounted by a maſs of vitrified matter, which bore evident marks of containing calcareous earth.

Thoſe who have devoted ſufficient attention to the affinities of iron and earths for carbon, will be ſurpriſed to find that, on this particular ſubject, the rude fabricators of ſteel in Hindoſtan have got the ſtart of our more poliſhed countrymen in the manufacture of ſteel.

Two bars of wootz were formed from this cake, and theſe in point of quality inferior to any of thoſe formerly produced. The appearance of the metal was more varied, leſs homogeneous, and contained more diſtinct laminæ with ruſty ſurfaces, than either of the two former cakes.

It

It appeared highly probable, from the obfervations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was fuch as would have formed a quality of fteel fofter than No. 1 and 2; but as fteel of fuch foftnefs requires a greater heat to fufe it, than when more fully faturated with carbonaceous matter, it is probable that the furnace had not been fufficiently powerful to occafion complete fufion of the whole mafs, and generate a fteel homogeneous in all its parts.

Forging 4th Cake.

Appearances on forging the cake of wootz, No. 4.

Both halves of this cake cut pleafantly, and with a degree of tenacity and refiftance, mixed at the fame time with foftnefs beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The refulting bars were nearly perfect. A flight fcale was obfervable upon the bar, from that quarter which contained the figure. The fracture was folid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a confiderable portion of one fide through the whole bar was in the ftate of malleable iron, and of courfe not capable of being hardened. It was a fubject of confiderable regret, that the cake the moft perfect and the moft tenacious of the whole, in the procefs of forging, fhould get an imperfection which rendered it ufelefs for the perfect purpofes of fteel.

Forging 5th Cake.

Appearances on forging the cake of wootz, No. 5.

The firft half of this cake cut uncommonly foft for wootz, but by cracking before the chiffel ftill exhibited a want of proper tenacity. The next half cut equally foft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, fometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and fcale, although the fracture exhibited a fair break of a light blue colour, and the grain was diftinctly marked, and free from yolks.

General Remarks.

Remarks. Wootz appears to be the product of a pecu-

The formation of wootz appears to me to be in confequence of the fufion of a peculiar ore, perhaps calcareous, or rendered highly

highly so by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other vessel or crucible, is equally presumable, in which the separated metal is allowed to cool; hence the crystallization that occupies the pits and cells found in and upon the under or rounded surface of the wootz cakes.

liar ore fused, and suffered to cool in a clay vessel;

The want of homogenity, and of real solidity in almost every cake of wootz, appears to me to be a direct consequence of the want of heat sufficiently powerful to effect a perfect reduction; what strengthens this supposition much is, that those cakes that are the hardest, *i. e.* that contain the greatest quantity of carbonaceous matter, and of course form the most fusible steel, are always the most solid and homogenious. On the contrary, those cakes, into which the cutting chissel most easily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

by an heat not sufficient for good fusion;

It is probable, had the native Hindostan the means of rendering his cast steel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for those purposes to which it is applied.

which is the reason why it was not cast in moulds.

The hammering, which is evident around the feeder and upon the upper surface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will most probably be slightly elevated, and the top of the cake partially covered with small masses of ore and steel iron, which the paucity of the heat had left either imperfectly separated or unfused. These most probably, to make the product more marketable, are cut off at a second heating, and the whole surface hammered smooth.

I have observed the same facts and similar appearances in operations of a like nature, and can account satisfactorily for it as follows.

The first portions of metal, that are separated in experiments of this nature, contain the largest share of the whole carbon introduced into the mixture. It follows of course, that an inferior degree of heat will maintain this portion of metal in a state of fluidity, but that a much higher temperature is requisite to reduce the particles of metal, thus for a season robbed of their carbon, and bring them into contact with the portion

portion firſt rendered fluid, to receive their proportion of the ſteely principle. Where the heat is languid, the deſcent of the laſt portions of iron is ſluggiſh, the maſs below begins to loſe its fluidity, while its diſpoſition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic maſſes of various diameters, riſing up for half an inch or more into the glaſs that covers the metal; theſe are neatly welded and inſerted into each other, and diminiſh in diameter as they go up. The length, or even the exiſtence of this feeder or excreſcence, depends upon the heat in general, and upon its temperature at different periods of the ſame proceſs. If there has been ſufficient heat, the ſurface will be convex and uniformly cryſtalline; but if the heat has been urged, after the feeder has been formed and an affinity eſtabliſhed between it and the ſteelified maſs below, it will only partially diſappear in the latter, and the head or part of the upper end of the feeder will be found ſuſpended in the glaſs that covers the ſteel.

The ſame or ſimilar phenomena take place in ſeparating crude iron from its ores, when highly carbonated, and difficult, from an exceſs of carbon, of being fuſed.

The diviſion of the wootz cake by the manufacturers of Hindoſtan, I apprehend is merely to facilitate its ſubſequent application to the purpoſes of the artiſt; it may ſerve at the ſame time as a teſt of the quality of the ſteel.

Experiments to aſcertain the comparative meaſure of carbon in wootz, by the quantity of lead it reduces from flint glaſs.

To aſcertain, by direct experiment, whether wootz owed its hardneſs to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common caſt ſteel, and of white crude iron, premiſing that in operations with iron and its ores, I have always found the comparative meaſure of carbon beſt aſcertained by the quantity of lead which was reduced from flint glaſs.

1ſt Cake.	Grains.
Fragments of wootz - - - - - -	65
Pounded flint glaſs three times the weight -	195

This mixture was expoſed to a heat of 160° Wedgwood, and the wootz fuſed into a well cryſtallized ſpherule of ſteel. A thin cruſt of revived lead was found below the wootz, which weighed 9 grains, or $\frac{138}{1000}$ the weight of wootz.

2d Cake

2d Cake.

	Grains.
Fragments of wootz - - - - - -	80
Flint glass, same proportion as above - -	240

Experiment to ascertain the comparative measure of carbon on wootz, by the quantity of lead it reduces from flint glass.

The fusion of the mixture in this experiment was productive of a mass of lead weighing 10 grains, equal to $\frac{1}{8}$th the weight of the wootz.

3d Cake.

Fragments of wootz - - - - -	75
Flint glass - - - - - - - -	225

The mass of lead precipitated beneath the steel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th Cake.

Fragments of wootz - - - - -	93
Flint glass - - - - - - - -	279

Lead obtained, precipitated from the glass by means of the carbon of the wootz $14\frac{1}{2}$ grains, equal to $\frac{156}{1000}$ the weight of the wootz.

5th Cake.

Fragments of wootz - - - - -	69
Flint glass - - - - - - -	207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6th. Cast Steel formed with $\frac{1}{60}$th part of its Weight of Carbon.

Fragments - - - - - - -	90
Crystal glass - - - - - - -	270

Lead revived $8\frac{1}{2}$ grains equal to $\frac{94}{1000}$ the weight of the steel introduced.

7th. White cast Iron dropt while Fluid into Water.

Fragments - - - - - - -	103
Crystal glass - - - - - - -	309

The fusion of this precipitated $23\frac{1}{2}$ grains of lead which is equal to $\frac{228}{1000}$ the weight of the cast iron.

 Recapitulation

Recapitulation of these Experiments.

Recapitulation of the experiments.

1st cake of wootz revived of lead		,132
2d	ditto	,125
3d	ditto	,120
4th	ditto	,156
5th	ditto	,102
Steel containing $\frac{1}{60}$ of its weight of carbon		,094
Cast iron		,228

Wootz contains more carbon than steel does, and less than cast iron.

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Its ore is probably very excellent.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

XI.

A Memoir on the Webs of Spiders. By C. L. CADET.*

On the natural and medical history of spiders.

SPIDERS have often excited the curiosity of naturalists and the attention of physicians. The former have successfully studied the habits and conduct of these insects; and notwithstanding the repugnance they naturally inspire, these accounts

* Abridged from the Journal de Physique, LVIII. 463.

have

have become interesting, from the industry with which they extend their webs for seizing their prey, and from observations on the multiplicity and arrangement of their eyes, which are geometrically disposed on a motionless head, in a manner conformable to their necessities. Their combats, the singularity of their amours, their sensibility for music, and their patience, all constitute subjects of wonder in the history of spiders. Physicians have examined whether their bite be really venomous, as is generally thought; and they have found only two species productive of danger, namely, the tarantula and avicularia of Cayenne. Swammerdam, Rossi, and Baglivi have left us little to wish for in this matter, as the effects of their bite and the remedies are both known.

Spiders' webs a popular remedy for slight wounds.

The webs of spiders are considered by the common people as a remedy for wounds; country people often apply them on cuts or slight wounds, and apparently with success. This property was not of sufficient importance to induce chemists to analyse the material; but as there has also been attributed to them a febrifuge virtue, superior in some circumstances to the bark, I have thought them entitled to a more particular examination. The following extract is taken from the *Journal d'Economie Rurale*, for Germinal, in the year XII.

Narrative of fevers cured by spiders internally taken.

"We have seen upwards of thirty years ago, a good prior, the curate of Batheren in Franche Comté, cure all the fevers of his parish, and of the neighbouring villages, by pills of a strange composition. He went into his barn and formed small pills with spiders' webs, by rolling them between his hands in the state he found them. He administered this remedy to his patients in white wine, and very seldom failed to cure. M. Marie de St. Ursin being chief physician of the Hotel de Dieu, of Chartres, treated a very obstinate fever in that hospital. He had employed bitters, the bark, and all the remedies of medical art without success, when one of the female attendants offered to undertake the case with a certainty of cure. When she was interrogated concerning her remedy, she refused to mention it. M. de St. Ursin therefore continued to attend his patient for some days; after which, having a good opinion of the attendant, he determined to put his patient under her care. There was no return of the fever after the first dose of the remedy. The physician supposed that the imagination of the patient, his confidence in a new remedy,

and particularly the secrecy, might have suspended the attack; and he waited, but to no purpose, for its return. The attendant encouraged by her success, consented to mention the remedy, which proved to be the same as that of the curate of Batheren."

Supposed to be effected by gelatine.

'The editor of the Journal here quoted, being struck with the new experiments of Seguin upon gelatine applied to the treatment of intermitting fevers, suspects that spiders' webs may contain a principle resembling animal jelly. The experiments of Cadet, while they overthrow this supposition, appear to him entitled to the attention of medical men.'

Analysis of spiders' webs.

"*Experiment* 1. Spiders' webs triturated in the cold with quick-lime, emit a slight ammoniacal smell. 2. Cold water by digestion on the webs, becomes of a red-brown colour; is slightly precipitated by infusion of nut-galls; is precipitated by acids; and this precipitate is again dissolved when the acids are saturated with ammonia. 3. Spiders' webs cleaned as much as possible from dust and foreign matters, were boiled in distilled water. The decoction smelled like champignons, and lathered by agitation. The undissolved matter was boiled in additional waters, until it gave out nothing more. All these waters being put together and evaporated, let fall their contents in successive pellicles; and at length, by gentle evaporation, a solid extract was had, nearly equal to half the weight of the spiders' webs. 4. The residue not dissolved in boiling water, was digested in alcohol. It gave a very deep orange-coloured tincture, which did not lather. Water being added, threw down a grey flaky precipitate, of a brown colour when dry, and little more than one hundred and seventieth part of the original webs. On hot coals it swelled up, smoaked, and took fire; and from its habitudes in these respects, and with the alkalies, it resembled a resin. The diluted alcoholic solution being then evaporated, afforded a residue slightly deliquescent, of a taste at first sweetish, and afterwards bitter, and in quantity nearly three times that of the resinous precipitate. 5. The insoluble residue after this treatment with water and alcohol, burned without swelling up, and emitted a small quantity of white fumes having the smell of burned wood. Neither the oxigenated muriatic, nor the sulphureous acids, discoloured it. It was soluble with effervescence in muriatic acid, which took up two-thirds and left

left a black paste. Ammonia separated a brown matter in small quantity from the clear solution; and this matter, when calcined, did not lose its colour. It was almost totally soluble in muriatic acid, and this solution gave a black precipitate with nut-galls, and a blue with alkaline prussiate. The fluid to which the ammonia had been added, gave a grey precipitate by pot-ash. This retained its colour when ignited, and was again soluble in muriatic acid with effervescence. 6. Caustic pot-ash poured on the residue of spiders' webs previously treated with water and alcohol, disengages a little ammonia, and partly dissolves the matter. An acid throws down from this solution a black pulverulent tasteless precipitate, which slightly puffs up by heat, and leaves by desiccation a brittle and apparently resinous matter. Its quantity is about one-twelfth of the exhausted matter made use of. It is partly soluble in volatile oils. Analysis of spiders' webs.

7. The aqueous extract of No. 3 being digested with alcohol, gave out one-seventh part. This alcoholic extract was brown, considerably deliquescent, and of a sharp taste. It swelled considerably on the coals, and at a certain period it burned rapidly, as if a nitrate were present. It effervesced briskly with sulphuric acid, giving out a white vapour of a muriatic smell. Potash and lime disengaged from this extract a strong ammoniacal smell, and the vapours were very sensible on the approach of muriatic acid. The extract having been incinerated, appeared by several experiments to contain muriate of lime and a sulphate. What remained of the aqueous extract after treatment with alcohol, was less deep in colour than before, had a pulverulent appearance, and slightly pungent taste. On hot coals it did not swell up, but left a very abundant precipitate. Strong sulphuric acid poured on this extract produced no sensible smell, and there was no production of ammonia when it was triturated with quick-lime.

8. Spiders' webs subjected to destructive distillation, gave first water slightly coloured, but becoming deeper as the process went on; and afterwards a black thick oil, with carbonated hydrogen and carbonic acid. A very sensible smell of ammonia was developed, and a residual coal was left, amounting to half the matter employed. The coal after incineration left two-thirds of its weight, half of which was taken up by muriatic acid, and the remainder seemed to be silex and coally matter.

Analysis of spiders' webs.

matter. The muriatic solution, during evaporation, deposited sulphate of lime. When spiders' webs were incinerated in an open vessel, the ashes were found to contain sulphate of lime, muriate of soda, and carbonate of soda. Muriatic acid applied to the residue took up more sulphate of lime; and when this solution was treated with ammonia and afterwards with pot-ash, it gave oxide of iron, a little alumine, and some lime. The undissolved part was silex.

9. Spiders' webs were almost totally dissolved in nitric acid amounting to six times their weight; carbonic acid and nitrous gas being disengaged. The solution when evaporated let fall crystals of sulphate of lime, and by continuing the evaporation, the yellow, bitter, deliquescent matter, which Welter calls *amer*, was afforded.

Component parts.

Hence the author concludes that spiders' webs are composed of, 1. A brown extract soluble in water, and not changeable in the air; 2. A resinous extract soluble in alcohol, and very deliquescent; 3. A small quantity of alumine; 4. Sulphate of lime; 5. Carbonate of soda; 6. Muriate of soda; 7. Carbonate of lime; 8. Iron; 9. Silex. The author thinks that the earths and earthy salts may be derived from the local situation of these insects, and that it is probable that the webs of garden spiders may not afford them. The two constant products to which he demands particular attention, are those obtained from the aqueous and alcoholic solutions. He thinks it desirable to try their medical powers separately. He supposes the resinous matter to be the same substance as under other circumstances forms the spiders' silk, and the wax which Mr. Accum has elsewhere mentioned as one of their products.

XII.

Information on the Mines and Manufactures of the East Indies, and other Subjects. By J. MACHLACHLAN, *Esq. of Calcutta.**

SIR,

Account of some receipts for dying.

SHOULD you think the enclosed receipts for dying the beautiful reds of the Coromandel coast can be of any use to the

* Soc. Arts, 1804; for which the silver medal was given.

the dyers of the united British kingdom, be pleased to lay them before the Society for the Encouragement of Arts, &c. that they may be published in the volume of their Transactions; if not, I trust you will excuse my troubling you with them. They were sent to me from Madras by a scientific friend, who had the several operations, detailed in them, performed in his own presence. I forwarded a copy of them, and a small quantity of the ingredients mentioned in them, to a friend at home, several years ago; but he dying about or soon after the time of their arrival, I never learned what became of them. It strikes me, however, that there is a considerable coincidence between the thread process and that which I have seen recommended by Mr. Henry, of Manchester, for dying the Adrianople or Turkey red.

Immense quantities of talc found in the hills in Bahar.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immense quantities of mica, talc, or Muscovy glass. The natives of this country and China make very splendid lanterns, shades, and ornaments of it, tinged of various fanciful colours; and it is also used by them in medicine. When burned or calcined, it is, I am told, considered as a specific in obstinate coughs and consumptions. When powdered, it serves to silver the Indian paper, &c. used in letter-writing; and, in fact, it is applied to numberless purposes. The bazar price of that of the best quality, split into sheets of about two lines thick, is six rupees the maund of 84lb. avoirdupois. If it could be applied to any useful purpose at home, it might go in part ballast of ships, and at a trifling expense. I enclose a small specimen of it, and am,

Sir,

Your very obedient servant,

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

N. B. The chaya, or red dye root of the Coast, is, I believe, known at home: as also the cashaw leaves, which are used as an astringent.

CHARLES TAYLOR, Esq.

Directions

Directions for dying a bright Red, four Yards of ¾ broad Cotton Cloth.

Instructions for dying cottons bright red by the Indian method.

1st. The cloth is to be well washed and dried, for the purpose of clearing it of lime and congee, or starch, generally used in India for bleaching and dressing cloths; then put into an earthen vessel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a short time over the fire.

2d. The cloth being taken out, washed in clean water, and dried in the sun, is again put into a pot with one ounce of myrabolans, or galls coarsely powdered, and a gallon of clear water, and allowed to boil to one half: when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully soaked in this, take it out, and dry it in the sun.

3d. Wash the cloth again in clear cold water, and dry it in the sun; then immerse it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the sun. The cloth, at this stage of the process, feeling rough and hard, is to be rolled up and beetled till it becomes soft.

4th. Infuse into six quarts of cold water, six ounces of red wood shavings, and allow it to remain so two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; soak in it your cloth twice over, drying it between each time in the shade.

5th. After three days wash in clean water, and half dry in the sun; then immerse the cloth into five gallons of water, at about the temperature of 120 degrees of Fahrenheit, adding 50 ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the sun to dry.

6th. Mix intimately together, by hand, about a pint measure of fresh sheep's dung, with a gallon of cold water, in which soak the cloth thoroughly, and immediately take it out, and dry it in the sun.

7th.

7th. Wash the cloth well in clean water, and spread it out in the sun on a sand-bank (which in India is universally preferred to a grass-plat) for six hours, sprinkling it from time to time, as it dries, with clean water, for the purpose of finishing and perfecting the colour, which will be of a very fine bright red. Instructions for dying cottons bright red by the Indian method.

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

CHARLES TAYLOR, Esq.

Directions for dying of a beautiful red, eight ounces of Cotton Thread.

1st. Put one gallon and a half, by measure, of sap-wood ashes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for use.

2. Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by measure, of sheep's dung, intimately mixed by hand in water; two pints of the above ley.—After mixing these ingredients well, pour the mixture gradually upon the thread into another vessel, wetting it only as the thread, by being squeezed and rolled about by the hand, imbibes it, continuing to do so until the whole is completely soaked up, and allow the thread to remain in this state until next day.

3d. Take it up, and put it in the sun to dry; then take a pint and a half of ash-ley, in which squeeze and roll the thread well and allow it to remain till next day.

4th. Squeeze and roll it in a like quantity of ash-ley, and put it in the sun to dry; when dry, squeeze and roll it again in the ley, and allow it to remain till next day.

5th. Let the same process be repeated three or four times, and intermit till next day.

6th. Ley the thread once, as the day before, and, when well dried in the sun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of ash-ley.—In this squeeze and roll the thread well, and leave it so till next day.

7th. Repeat the process of yesterday, and dry the thread in the sun.

Instructions for dying cottons bright red by the Indian method.

8th. The ſame proceſs to be repeated.

9th. Firſt repeat the aſh-ley proceſs three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: On pint of ſheep-dung water; one gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and dry it in the ſun.

10th. Repeat the ſame proceſs.

11th. Do. Do.

12th. Do. Do.

13th. Do. Do.

14th. Do. Do.

15th. Waſh the thread in clean water, and ſqueeze and roll it in a cloth until almoſt dry; then put it into a veſſel containing a gill of powdered chaya root, one pint by meaſure of caſhan leaves, and ten pints of clear water; in this liquor ſqueeze and roll it about well, and allow it to remain ſo till next day.

16th. Wring the thread, and dry it in the ſun, and repeat again the whole of the 15th proceſs. leaving the thread to ſteep.

17th. Wring it well, dry it in the ſun, and repeat the ſame proceſs as the day before.

18th. Do. Do.

19th. Do. Do.

20. Wring and dry it in the ſun, and with the like quantity of chaya root in ten pints of water, boil the thread for three hours, and allow it to remain in the infuſion until cold.

21ſt. Waſh the thread well in clear water, dry it in the ſun, and the whole proceſs is complete.

J. MACLACHLAN.

Calcutta, Oct. 4, 1803.

SCIENTIFIC

SCIENTIFIC NEWS.

Imperial Academy of Sciences at Petersburgh.

THE Vice Admiral Tchitchagoff, Minister of the Marine department, has forwarded to the Academy a question on the resistance of fluids, and its application to naval architecture, for the solution of which that department will bestow a reward of 1000 ducats of Holland, or 462*l.* 10*s.*

Prize of the Russian Imperial Academy.

The academy being desirous of seconding the patriotic views of the marine department, decided on the publication of a program in the following terms.

Prize proposed by the department of the Marine, on the 1st of *July*, 1804.

It is proposed, that of the two theories of the resistance of fluids proposed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, one or the other of them, for example, that of Don Juan should be corrected and improved to such a degree, as to afford results that shall differ from the results of experiment, by so small a quantity, as may be practically neglected without sensible error:—Or otherwise, if these theories cannot be corrected, it is proposed, that a new theory should be established and applied to naval architecture, which shall lead to conclusions of the same degree of accuracy;—Or otherwise, lastly, if it should be impossible to establish such a theory, it is proposed, that from experiments at least, there should be deduced a formula resembling those which have been given by Messrs. Bossut and Prony; and such that it shall be not only more conformable to experiments than those formulas, but that it shall lead as nearly as possible to the conclusions drawn from experiments, even when the formulæ shall be applied to naval architecture.

The subject. To improve the theories of the resistance of fluids; or to establish a new theory; or to deduce a formula from experiment.

For the satisfactory solution of this problem the department of the marine has appointed a prize of 1000 Ducats of Holland, and has fixed a term of two years to be reckoned from the date of this program. After the expiration of that term, no memoirs addressed to the academy will be received on this subject, the time appointed being sufficient for those new experiments which the solutions in question render indispensa-

One thousand ducats, or 462*l.* 10*s.*

bly

bly necessary. The memoirs forwarded to the academy must be written in a distinct legible character, either in the French English or Russian language.

(Signed in the original) PAUL TCHITCHAGOFF.

Terms and conditions for receiving memoirs.

In the invitation accompanying this program, the Academy requires men of science who intend to make application for this prize, to address their memoirs to the perpetual secretary of that body, before the 1st of July, 1806, and that the writer should clear the post charges as far as the regulations of their respective countries will allow. The customary mode of marking the memoirs with a device or motto, and sending at the same time a sealed letter, having the same device, and containing the name and residence of the author, is also to be adopted in the present instance. The memoirs will be examined before the expiration of the term of concurrence, by the Department of the Marine and by the Academy; the latter of whom will publish the judgment they shall adopt, and the Department of the Marine will crown by the payment of the prize, the labours of that author who shall have satisfied the conditions of the program.

Prize concerning Light, proposed by the Imperial Academy of Sciences for the Year 1806.

Prize concerning light.

The usual theories that light is projected matter; or mere undulation of a fluid;—

—or it is a chemical principle.

Five hundred roubles, £.112 10 0.

The question.

After an introduction, in which a concise statement is given of two theories respecting the nature of light, the one ascribed to Newton, which supposes light to consist in the emanations of matter from luminous objects, and the other ascribed to Euler, which deduces the effects from vibrations of a peculiar elastic fluid,—they proceed to state rather more fully a chemical hypothesis of Lavoisier, who not only considers light as caused by a peculiar matter, but also that this matter is subject to the elective attractions, so as by its combinations and disengagements to produce an extensive series of phenomena, which are thus accounted for. It is principally with a view to develope this last hypothesis that they have proposed a prize of 500 roubles (£112 10 0.)

"For the most instructive series of new experiments on light, considered as matter; on the properties which may with justice be ascribed to it; on the affinities which it shall appear to have with other bodies, whether organic or inorganic, and on

on the modifications and phenomena which are manifested in those substances by virtue of the combinations in which the matter of light may have entered along with them."

After proposing this question, the Academy proceeds to explain, by observing that without entering into any historic discussion, or the objections which have been opposed to this hypothesis, nor the researches already made with a view to develope traces of chemical action between light and bodies in the different modifications of natural phenomena,—the enquiries here proposed may not be unusefully extended to the galvanic fire, of which the dazzling brilliancy when large piles are made to act upon coally matters, in some respect imitates the solar light. The Academy has chosen to enunciate the subject of their prize in a general way, in order that philosophers may not be in any respect impeded as to the points of view from which they may be disposed to contemplate and to treat so difficult a subject, which has scarcely yet been entered upon, though so eminently worthy of attention from the cultivators of natural science. Remarks by the academy.

The memoirs are to be written either in Russian, French, English, German, or Latin, and forwarded to the perpetual secretary of the Academy, sealed up, with device and indicatory billet, as mentioned with regard to the former prize. No memoirs will be received after the 30th of April, 1806, inclusive, and the author of that memoir which in the judgment of the Academy shall have merited the prize, shall be proclaimed in the public meeting of the following month of July. The successful memoir becomes the property of the Academy, and must not be printed without their formal permission. The other treatises will be delivered to the respective authors, on application to the secretary, either personally or by procuration. Conditions and terms.

Voyages of Messrs. Humboldt *and* Bonpland.

Messrs. Levrault, Schoell and Company circulated, at the beginning of the present year a prospectus of the voyage of Messrs. Humboldt and Bonpland, the publication of which is committed to them: "the travellers, they observe, have in general re-written all their observations, whatever might have been the object, in the narrative of their voyage. Mr. Humboldt has thought it proper to follow another course, and to treat Voyages of Humboldt and Bonpland.

Humboldt's travels.

treat separately all those objects which considerably differ in their nature. He has determined accordingly to publish in detached collections, all that more particularly belongs to Astronomy, Geology, Botany, Zoology, &c. and his voyage, properly so called, will embrace all that relates to General Physics, the Origin of Nations, their Manners, their Intellectual Culture, Antiquities, Commerce, and Political Economy. Upon this part of his observations and the history of his voyage, he will not at present publish more than a narrative, under the title of *Relation abrégé*, &c. or an abridged Relation of a Voyage to the Tropics, performed in the interior of the new continent during the years 1799, 1800, 1801, 1802, and 1803.

It is agreed by Messrs. Humboldt and Bonpland, who are connected by the most intimate sentiments of friendship, and have shared together in all the fatigues and dangers of this voyage, that the whole of their publications shall be in their joint names. The preface to each work will announce to which of them the several parts are respectively to be ascribed. The list of works speedily to appear are as follows:

1. The abridged Relation of the Voyage, in quarto; promised in July, 1805.

2. A Collection of Astronomical Observations and Admeasurements made on the new continent; promised in 1805.

3. An Essay on the Geography of Plants, or a Philosophical Sketch of the Equinoctial Regions; founded on observations made from the 18th degree of south latitude, in the years before mentioned, with one large plate, coloured; promised in June, 1805.

4. Equinoctial Plants, collected in Mexico and the Isle of Cuba, in the Provinces of Caracas, Cumena and Barcelona, on the Andes of New Grenada, Quito and Peru, and on the banks of Rio Negro, Oroonoko and the River of Amazons, with plates engraved by *Sellier*, in folio; the first number to appear in April, 1805.

5. A Collection of Observations of Zoology and comparative Anatomy, made in a Voyage to the Tropics; with plates engraved by *Bouquet*, coloured or not, at the option of the purchaser. The first number to appear in May.

N. B. All these works will bear the general title of *Voyage de M. M. Alexandre de Humboldt et Aimé Bonpland*, and will form

form a collection of the same size and type, except the Equinoctial Plants, which are larger. Subsequent notices of the price and publication are to appear in the journals.

Fish ejected from Volcanoes.

Volcanic eruption of fishes.

AMONG the great number of facts which Humboldt has collected in his voyage, the following lately communicated to the National Institute is very curious. Several volcanoes of the Cordilleras of the Andes occasionally throw out eruptions of mud mixed with large volumes of fresh water, and what is most remarkable, an infinite number of fishes. The volcano of Imbaburo, among others, threw out at one time so great a number near the town of Ibarra, that their putrefaction occasioned disorders. This phenomenon, astonishing as it appears, is not even extraordinary, but, on the contrary, of considerable frequency, so that the facts are authentically preserved in the public registers, along with those of earthquakes. It is more particularly singular that these fish are not at all injured, though their structure is very soft. They do not even appear to have been exposed to a high temperature; for the Indians assert that they sometimes arrive at the foot of the mountain still living. These animals are sometimes thrown out of the mouths of the crater and sometimes through lateral clefts; but always at the height of 12 or 1300 toises or fathoms above the surrounding plains. Humboldt thinks they are produced in lakes situated at that height within the crater; and it is a confirmation of this opinion that the same species are found in the brooks which run at the foot of the mountains. It is the only species which subsists at the height of 1400 toises. It is a new species to naturalists. Humboldt made a drawing of it on the spot, and gave it the name of *Pimelodrus Cyclopum* or *thrown by the Cyclops.* It will be found in the first number of his Zoology.

Water formed by Mechanical Pressure.

Combustion of oxygen and hydrogen by mechanical pressure.

IN a sitting of the French National Institute at the commencement of the present year, M. Biot read a note on the formation of water by mere compression. The experiment of forming water out of its component parts oxygen and hydrogen by burning those gases by the electric spark is well known. M. Biot has succeeded in determining this combination independently of electricity, by rapidly compressing a mixture of the

the two gases included in the syringe of an air-gun. The compression which forces the particles of gas together, causes them to give out a sufficient quantity of heat to set them on fire. Some caution is requisite in repeating this experiment, which is not without danger. Out of three times that M. Biot made it, there were two in which the brass cap of the pump and the barrel itself, which was iron, were broken by the explosion.

Malleability of Zinc.

Zinc is malleable while heated between 210° and 300°.

A VERY curious and useful discovery has been made by Messrs. Charles Hobson and Charles Sylvester, both of Sheffield, that zinc is in fact a malleable metal. The laminability of this metal to a certain considerable degree, has long been known; but it was not suspected that it is capable of being forged and drawn into wire. They have found that at a temperature between 210° and 300° of Fahrenheit zinc yields to the hammer, and also that it may be wire-drawn or laminated by keeping it at this temperature during the mechanical operation. An oven or a hollow metallic vessel kept at a due heat may be used for the pieces, in the same manner as the Smith's forge is used for iron and steel. It appears that the zinc, after having been thus annealed and wrought, continues soft, flexible and extensible, and does not return to its former partial brittleness, but may be bended and applied to the uses for which zinc has hitherto been thought unfit, such as the fabrication of vessels, the sheathing of ships, and numerous other important applications. I have seen a chased or stamped figure raised at one stroke in thin zinc; which is, I think, as much elevated as it could have been in copper.*

Palladium.

Palladium.

BY a letter from Messrs. Knight, of Foster-lane, I am informed that the new metal, palladium, may be purchased at their warehouse.

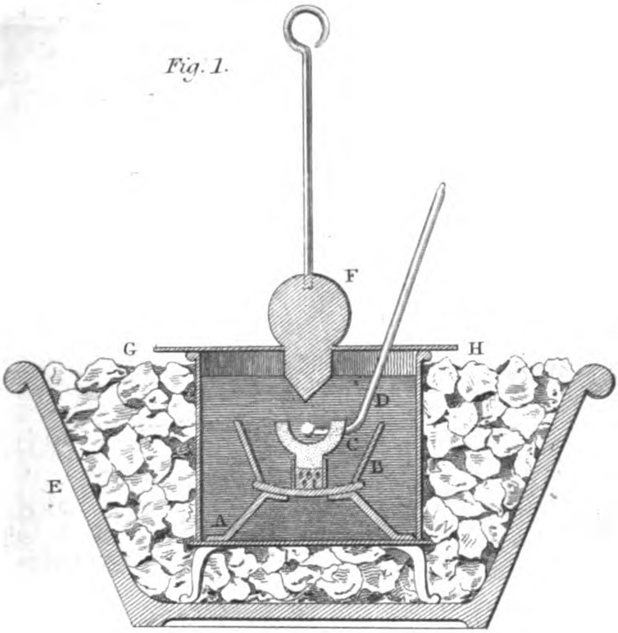
Erratum corrected.

MR. GREGORY begs leave to correct a mistake which occurs in his paper on horse-powers, occasioned by his inadvertently copying from some of his former observations on that subject the fraction $\frac{3}{4}$ instead of $\frac{2}{3}$ for the approximate value of the exponent x: this makes a change in the results of the computation at page 148; for $(9-3)^{\frac{2}{3}} : (9-4.4)^{\frac{2}{3}} :: 130 : 80{\cdot}58$, or nearly $80\frac{1}{2}$ lb. instead of $71\frac{1}{2}$, as there given. He also points out a press error at p. 153, where the symbols of multiplication in each of the theorems should have been signs of addition.

* The inventors have obtained a patent.

New Experiments by Count Rumford to prove the maximum of Density in Water above 32° fahr.

Fig. 1.



Chevalier Edelcrantz's Valves for Steam Engines.

Fig. 2.

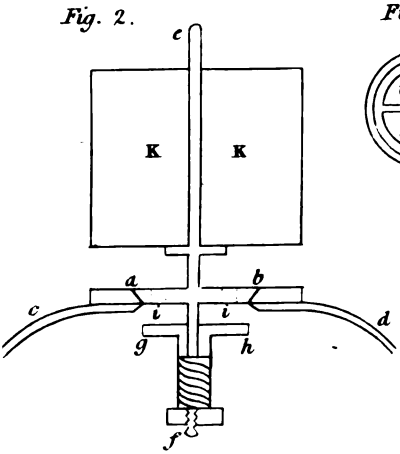


Fig. 3.

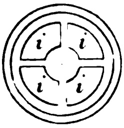
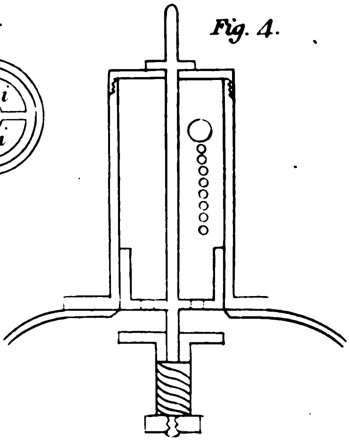


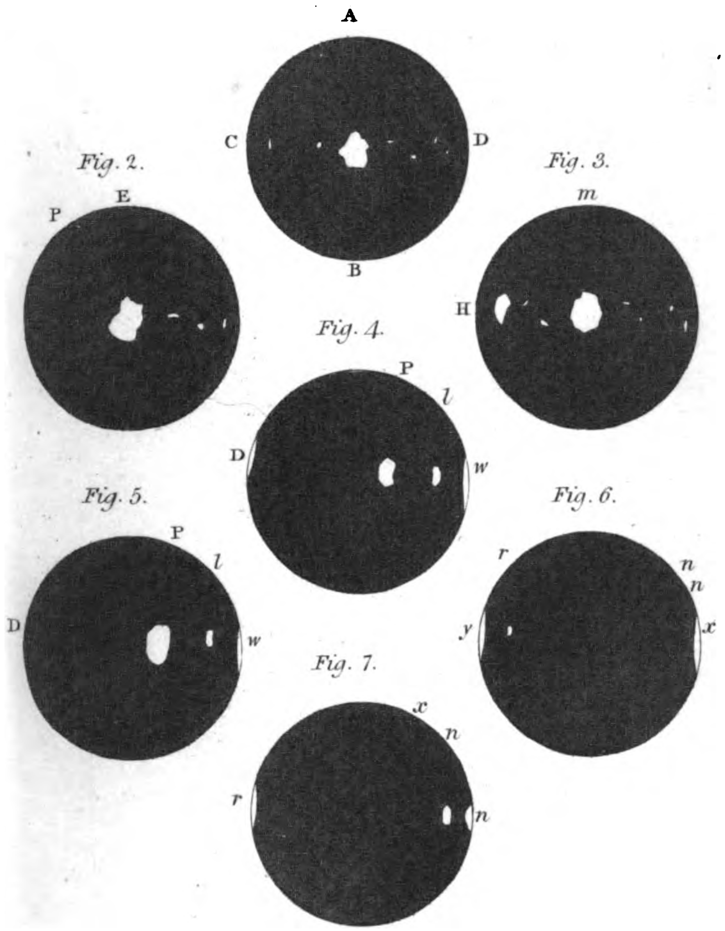
Fig. 4.



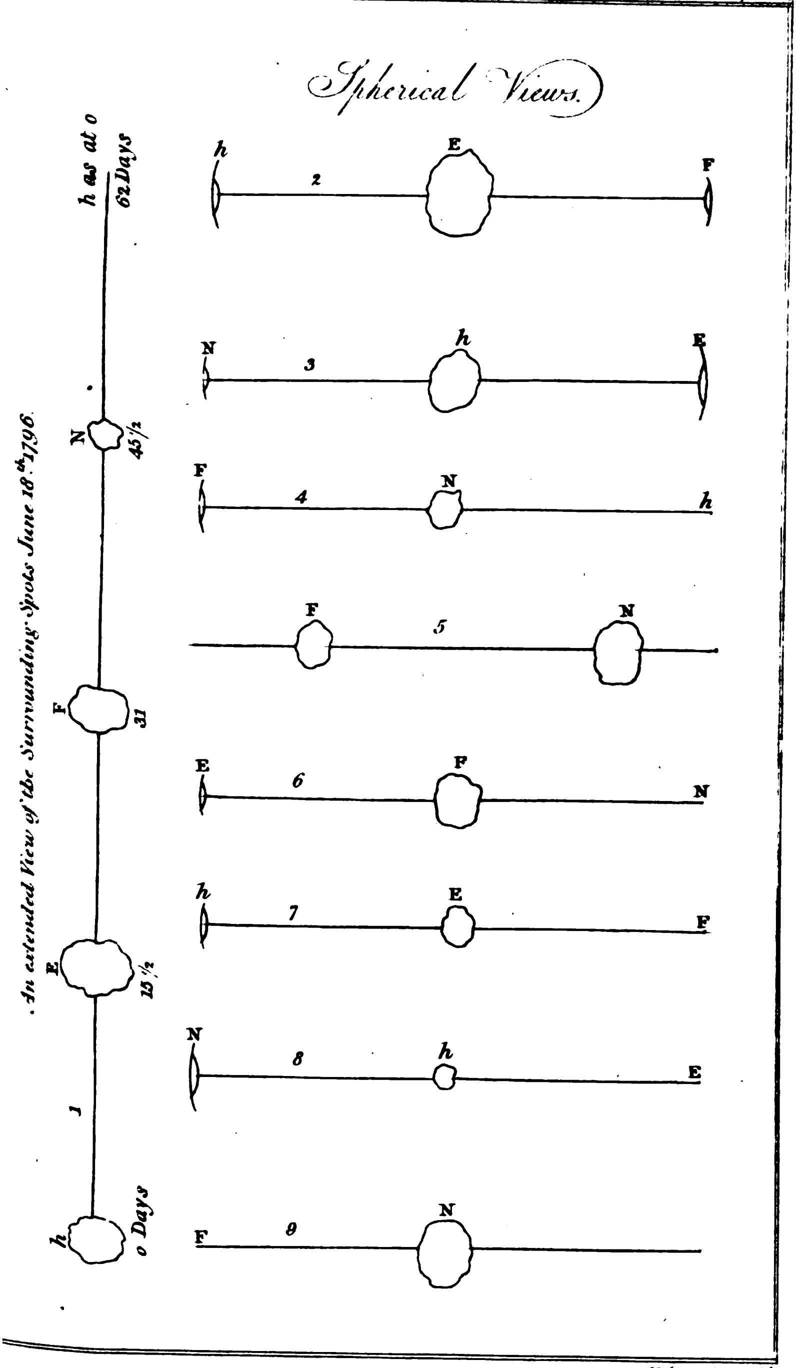
Mutlow sc. Russell C.t

Variable Star in Sobieski's Shield.

Fig. 1.



Mutlow Sc. Russell Ct.



Mutlow Sc. Russell Ct.

Mr. Harrison's Press for Botanical Specimens.

Fig. 1.

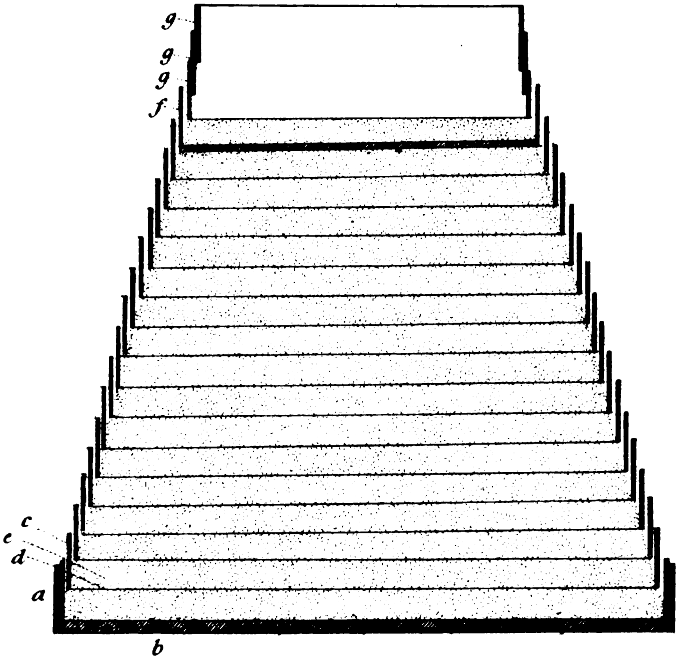


Fig. 2.

Fig. 3

Mutlow Sc. Russell Ct.

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THE END OF THE ELEVENTH VOLUME.

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1805

PREFACE.

THE Authors of Original Papers in the present Volume, are, Davies Giddy, Esq. M. P.; Mr. O. Gregory; Mr. William Close; Mr. John Dalton; W. N.; Mr. Charles Young; Mr. J. Stodart; Count Rumford, V. P R. S.; Mr. G. I. Singer; J. P.; T. S. Traill, M. D.; G. Cumberland, Esq.; Mr. S. Clegg; Mr. Dalton; T. I. B.; T. Plowman, Esq.; F. A.; N. D. Starck, R. N.; John Gough, Esq.; A Correspondent; An Enquirer; Mr. Arthur Woolf; Mr. Elizur Wright; Thomas Northmore, Esq.; Dr. Beddoes; Mr. Bancks.

Of Foreign Works, Professor Proust; J. H. Hassenfratz; G. A. Lampadius; Mr. Prony; Beaunier; Gallois; J. B. Richter; Mr. Ritter; Citizen Bernoulli; Vauquelin; C. A. Prieur; Guyton; Morveau; Chaptal; Dr. Valli; M. Vaucher; M. Hauffman; Biot; Thenard; Descotils; C. L. Cadet; Professor Pfaff; Tromsdorf; M. Achard; M. Steinacher; M. Giobert; Humboldt; M. Lagoux de Flaix; M. Dodun.

And of English Memoirs abridged or extracted, Humphrey Davy, Esq. F. R. S.; Mr. Rose; Benj. Smith Barton, M. D.; Mr. John Heckewelder; J. C. Curwen, Esq. M. P.; Mr. William Bartram; Thomas Andrew Knight, Esq.; Rev. D. Pape; William Herschel, L. L. D. F. R. S.; Charles Hatchett, Esq. F. R. S.; T. C. Hope, M. D. F. R. S. Ed.

Of

PREFACE.

Of the Engravings, the Subjects are, 1. Apparatus for raising Water, by Mr. Close. 2. Furnace for bending Wood. 3, 4. Blowing Engine by the Fall of Water at Poullaouen. 5, 6. Apparatus for analysing Vegetable Soils, by H. Davy, Esq. 7. Diagrams to explain the Doctrine of Heat, by Count Rumford, V. P. R. S. 8. Magnified Representation of Insects found on Corn, by G. Cumberland, Esq. 9. An improved Steam Engine, by Mr. Clegg. 10. Simple Register Thermometers. 11. The Framing employed to raise the Roof of Clapham Church. 12. An improved Sheep Fold, by T. Plowman, Esq. 13. Applicative Compass, by Capt. Starck, R. N. 14. Section of a Drain, by J. C. Curwen, Esq. 15. Diagrams to illustrate a Series of Propositions respecting a Division of the Circle, by Mr. John Gough. 16. Schemes exhibiting the Positions of the new Planet Juno, by Dr. Herschel. 17. Mr. Pape's Improvement of Rye Harbour. 18. A new Air Pump, by Mr. Wright of America. 19. New Valve and Parts of a condensing Syringe for the Gases, by Mr. Cuthbertson, with which Mr. Northmore's Experiments were made. 20. A very simple and improved Graphometer, by Mr. Bancks.

Soho Square, London, January 1, 1805.

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SUPPLEMENT.

ADVERTISEMENT.

THE Patrons and Friends of the Philosophical Journal are respectfully informed, that this work will in future consist of six sheets of matter instead of five in each number, together with a supplementary number to each volume; and that the plates will be so managed as to contain a larger number of subjects executed in the best stile. These improvements, though attended with additional expence, are such as the Editor has thought it his duty to adopt; in order that the very extensive communications with which the Journal has been honored, might not prevent him from giving all the foreign discoveries and such other general intelligence as the nature of the plan demands. It will easily be seen that the additional copy will by this means amount to one half more than the former quantity; and it is unnecessary to point out the great advantages which must result from such an addition. The Editor takes this occasion to repeat his acknowledgments for the encouragement which has been given to his exertions, particularly within the last twelve months, in which the sale has nearly doubled. The quantity of original matter continues to increase, and amounts to more than half of the whole work. Great part of the remainder consists in foreign articles never before published in this country, together with some extracts and abridgments from our best academical transactions. The whole publication may therefore be considered as original, since it is never made up by extracts from the periodical works of this country, which, on the contrary, very frequently copy from its contents.*

* It has been necessary to reprint a considerable part of the former volumes, in order to make complete sets, which may now be had, or any single numbers or volumes, from the commencement.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1805.

ARTICLE I.

Letter from DAVIES GIDDY, *Esq. M. P. describing a singular Fact of the invisible Emission of Steam and Smoke together from the Chimney of a Furnace; though either of them, if separately emitted, is visible as usual.*

To Mr. NICHOLSON.

SIR, *Clifton, August* 6, 1805.

Peculiar facts observed in working one of Trevithick's steam engines.

TRAVELLING, and a variety of occupations, have hitherto prevented me from sending you an account of the circumstances observed by myself and others, during the working of an engine on Mr. Trevithick's construction, at Merthyn Tidwell in South Wales, and which I had the pleasure of relating to you, some time since, at Soho Square. I now transmit a statement of the facts, avoiding all comments or attempts at explanation.

The steam engine was adapted to move a carriage, and every part made light and simple.

Mr. Trevithick having adapted his steam engine to the purpose of moving waggons, contrived every accessary part as light as he possibly could, and as little inconvenient to persons who might assist, or witness an experiment. The flue for conveying off the smoke, and affording a draft, was made

The steam was (after work) thrown into the chimney many feet from the fire.

Neither smoke nor steam were visible.

When the smoke was shut off the steam was visible, and when the steam was shut off the smoke was visible.

The draft up the chimney was increased by the admission of the steam.

of rolled iron; and the steam, which wholly escapes from these machines uncondensed, was conducted into the same tube, about a foot above its insertion into the boiler; therefore many feet from the fire, and beyond the register. When the engine began to move, it was soon remarked that neither steam nor smoke were seen to issue from the flue: and when fresh coal was added, nothing more than a faint white cloud became apparent, and that only for a short time; nor were drops or mist visible any where. It was proposed, that the register should be slowly closed; and as this was done, a condensation of steam manifested itself at a small distance from the chimney, and finally appeared in the same quantity, as if it had proceeded immediately from the boiler. The experiment was then reversed. The steam was gradually confined to the boiler; when smoke became more and more visible, till it equalled in quantity and appearance that commonly produced by a similar fire: and these trials were alternated a great number of times, with unvarying success. Lastly, it became a matter of speculation, whether or in what degree the draft was affected by the admission of steam into the flue. To ascertain this, every one present looked as attentively as possible into the fire-place; while the engine moved at the rate of a few strokes in a minute; and all agreed in declaring, that the fire brightened each time the steam obtained admission into the chimney, as the engine made its stroke.

I am, Sir,

Your very faithful humble servant,

DAVIES GIDDY.

II.

On a Meteoric Stone that fell in the Neighbourhood of Sigena, in Arragon, in 1773, by Professor PROUST *.

Stones have fallen from the atmosphere in various ages,

THE author begins his paper by some previous historical facts. No one now questions, that stones have fallen from the atmosphere in different parts of the world. The ancients

* Abridged from the Journal de Physique, for March 1805.

mention it as having occurred at various times, and later ages have recorded the time and authenticated circumſtances of ſeveral ſuch incidents. In our own days, ſtones or mineral bodies termed meteoric, have been collected in the Eaſt Indies, America, Scotland, England, France, Italy, Hungary, and laſtly in Spain: and that nothing may be wanting in future to convince thoſe who refuſe their aſſent to the united teſtimony of all ages and all countries, nature appears to have purpoſely ordered a repetition of this ſurpriſing phenomenon: no longer ago than the 26th of April, 1803, a ſhower of theſe ſtones covered a ſpace of ground two miles long and above a mile broad, near l'Aigle in Normandy. The French Inſtitute immediately nominated a commiſſioner, to examine into the fact on the ſpot, to take the depoſitions of witneſſes, compare them with the circumſtances, and bring ſome of the ſtones to Paris.

and in all parts of the world.

Shower of ſtones in 1803.

As the firſt thing to be done with a new mineralogical ſubſtance, is, to analyſe it, the Preſident of the Royal Society of London, and ſeveral other gentlemen who had ſuch ſtones in their collections, put them into the hands of Mr. Howard, a Member of the Society, that he might ſubject them to chemical examination. He found to his great ſurpriſe, that all theſe ſtones, from the remoteſt quarters of the globe, contained the ſame principles, differing only in proportion; and, what was ſtill more ſtriking, that they all contained iron combined with nickel, a compound to be met with among none of the minerals in any part of the globe with which we are acquainted. Vauquelin has ſince confirmed by repeated experiments, the accuracy of Mr. Howard's obſervations. All men of ſcience have hence been led to conclude, that theſe ſtones muſt have a common origin; but whence they originate is the queſtion. Do they belong to that earth on which they fall? are they formed in the atmoſphere itſelf? or have they been projected from lunar volcanoes? On theſe points men's ſentiments are divided; and the arguments have been collected by Dr. Izarn, in his *Lithologie Atmoſphérique*.

Analyſis of many of theſe bodies by Mr. Howard,

who found them all ſimilar in their compoſition.

They have therefore a common origin,

but whence?

One of theſe ſtones has been in the royal collection at Madrid ever ſince 1773. This the miniſter has allowed Mr. P. to analyſe, leaving the principal part of it ſtill in the collection for the ſatisfaction of the curious. The following letter was ſent with it to Don Manuel de Roda, Miniſter of State, by the captain-general of Saragoſſa.

One in the collection at Madrid.

Account of its fall.

"In November last an extraordinary occurrence, said to have happened on the Seventeenth of that Month in a ploughed field at Sena, a village in the district of Sigena, was the subject of conversation in this city.

"The sky being perfectly serene, three reports resembling those of cannon were heard, and followed by the fall of a stone weighing nine pounds and one ounce, at a little distance from two labouring men. One of them went up to it, but the strong smell it emitted stopped him a moment.

"Recovering from his surprise he went nearer, raised it up with his spade, and waited till it was sufficiently cold for him to carry it to the village, where he delivered it to the priest.

"From inquiries made immediately afterwards on the spot, and among the people in the neighbourhood, it appears, that the noise in the air and fall of the stone were not accompanied with any storm, or with lightning."

Another meteoric shower of stones that fell at Roa in Spain, in 1438,

To bring into one view all that is yet known of stones falling in Spain, the author subjoins a letter of the Bachelor Cibdadreal, on those that fell in the village of Roa, near Burgos, in 1438.

in the view of the King of Spain.

"While the King Don John and his court were hawking near the village of Roa, the sun was concealed behind white clouds, and bodies resembling gray and blackish stones were seen to fall from the air, of such bulk as to occasion the greatest surprise.

The ground was covered with them.

"After this phenomenon had continued for an hour the sun re-appeared, and the falconers immediately rode to the place, which was not above a mile distant. They brought back information to the king, that the ground was so completely covered with stones of all sizes as not to be visible.

"The king would have gone thither, but his courtiers prevented him, observing, that a place chosen by Heaven for the theatre of its operations might not be free from danger, and that he had better send some of his attendants. Gomez Bravo, the captain of his guards, undertook the office. He brought four of the stones to Roa, whither the king had already retired. They were of considerable size: some were round, and as large as a mortar, others like pillows and *half-fanegue* measures (such as contain about 45lbs weight of corn,) but what was most astonishing, was their excessive lightness, since

These stones were very large,

extremely light,

4 the

the largest did not weigh half a pound. They were of such a tender texture, that they resembled the foam of the sea condensed more than any thing. You might strike on them with your hand without fear of bruise, or pain, or the slightest mark. The king has ordered some to be sent you, &c." — and of a tender texture.

It seems from this description, that these stones must have been very different from those of the present day.

The stone of Sigena, when delivered to Mr. Proust, weighed six pounds ten ounces. With it was a piece of three or four ounces, the only one remaining of those that had been broken from it by curious persons. It was interspersed with spots of rust, both externally and internally, owing probably to its having been immersed in water to try the effect of that fluid on it. From these however, some instructive inferences may be drawn respecting the native place of these stones. — The stone of Sigena described. Interspersed with rusty spots.

Its shape is an irregular oval, seven or eight inches long, four or five broad, and four in its greatest thickness. One side is flattish, a little depressed in the middle, and very round on the edges: the other is an obtuse triedral pyramid with unequal sides, greatly rounded at the summit and on the edges *. It appears to have had the black vitreous crust common to stones of this kind, though from its fragility the greater part has fallen off in passing through many hands and receiving occasional blows, so that none remains except in the hollow of the base, and a little on the faces of the pyramid. — Its figure. It had a vitreous crust.

On examining this crust it is easy to see, that it must have been the effect of heat subsequent to the formation of the stone, and unquestionably very powerful though momentary; since the metallic and sulphureous particles immediately beneath the crust had not time to change colour, or even lose their lustre. — This must have been produced by violent momentary heat since its formation.

It has all the porosity of an aggregated mass of sandy particles without any cement, so that the breath will easily pass through a piece held between the teeth. It will not strike fire with steel, and the same may be said of the pyrites it contains. — It is porous, not very hard,

Its colour is a uniform bluish gray, like that of a black substance enlightened by a white: it is the hue of an earthy compound tinged by iron oxided at a *minimum*. — of a bluish gray.

* Does this description agree with what is said above, that several pieces have been broken from it? Apparently above a quarter of the stone, on comparing its original and present weight. J. C.

The

It is a sandy mass interspersed with metallic and sulphureous particles. Its granules are crystalline.

The stone itself is a sandy mass, formed of rounded oval grains, the largest of which are scarcely bigger than hemp-seed, among which are interspersed metallic and sulphureous particles with all their primitive lustre, and particularly with that light tint of kupfernickel observed in the other stones. On examining the earthy grains by the microscope, we perceive, that, far from having been fashioned by the movement of water, they are globules rough with crystalline or reflecting points, so that they can by no means be confounded with sand.

Heat deepened its colour, and oxided the metal.

A piece of about two inches being exposed to a red heat in a crucible for half a quarter of an hour was much changed: the sandy globules became of a darker gray, and the metallic particles, divested of their lustre, were visibly oxided.

Greater heat fused it.

About two ounces were heated for half an hour in a forge fire, which converted the stone into a semivitreous mass, blackish, and slightly porous. It did not appear to have effervesced much previous to fusion, and was interspersed with globules of iron, which had not time to descend, though upwards of a hundred grains of a regulus were collected at the bottom.

It contained much magnetic iron, combined with nickel.

The iron attractable by the magnet was not uniformly mixed in the stone, as from some parts 22 in the 100 were extracted, from others not more than 17.

This iron was combined with nickel in the proportion of about 3 per cent. No nickel was discoverable in any other part of the stone.

Constituent parts of the remainder.

After this alloy was separated by the magnet, the remainder of the stone was found by analysis to consist of,

Iron sulphurated at a *minimum* -	12
Black oxide of iron - - -	5
Silex - - - -	66
Magnesia - - - -	20
Lime and magnesia in quantities too small to be appreciated - -	
	103

New hypothesis. Their origin probably in the polar regions,

On considering the rapid alteration of these stones by moisture, for a fragment kept twelve hours under water was taken out covered with spots of rust, which distinguished the grains of alloy from the sulphureous particles with which they were before confounded;—it is obvious, according to the author

author, that they cannot fubfift in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a folid mafs, and iron cannot ruft, he thinks we may reafonably look to thefe regions as the native place of fuch bodies. In this he infifts there is nothing impoffible, or even improbable. And why fhould thofe meteors, he demands, of which we know neither the origin, the combuftibles that afford them aliment, the impulfe by which they are moved, nor the nature of the lines they defcribe in their courfe, be lefs capable of tearing them from fome part of the globe, than of forming them, contrary to all phyfical probability, from elements which the atmofphere can neither create nor hold in folution?

whence they are conveyed to other parts by meteors.

III.

Farther Remarks on Mechanic Power, in Reply to Mr. J. C. Hornblower. In a Letter from Mr. O. GREGORY, *Royal Mil. Academy.*

To Mr. NICHOLSON.

SIR,

I AM forry to be under the neceffity of troubling you with a few obfervations for infertion in your Journal, in confequence of being called upon by Mr. Hornblower, as though it were to defend fome newfangled doctrine, when the pofitions in my former letter, which that gentleman thinks proper to cenfure, are in perfect conformity with the principles affumed or demonftrated by every correct writer on mechanical philofophy fince it has been placed upon its proper bafis in the *Principia* of Newton. The fubject I am now invited to difcufs, has fo frequently been exhibited in the cleareft light by various authors, both in England and on the Continent, that I fhould not think myfelf juftified in occupying many of your pages by an elaborate differtation; out of regard, however, to fo refpectable a correfpondent as Mr. H. I cannot help entering a little into the difcuffion, though I am, I confefs, quite unable to afcertain whether his laft letter is meant to oppofe my former remarks, and thofe of Profeffor Robifon with ferious arguments, or is merely intended as a *Jeu d'efprit*.

Prefatory remarks.

It

It will not, I hope, be expected that I should point out in what inſtances Mr. H's remarks appear to me completely irrelative to the ſubject in hand; or thoſe in which he ſeems to have miſunderſtood the arguments of the late learned Profeſſor:—ſuch a procedure would only lead into farther diſcuſſion, whilſt I feel ſolicitous to avoid it, from a conſciouſneſs that it would be very unintereſting to moſt of your readers. I ſhall ſtrive to confine myſelf, therefore, to ſuch of Mr. H's enquiries as bear upon the point in diſpute, and for the ſake of condenſing my labour, I ſhall begin with that in his poſtſcript.

Are animal exerti n and mechanic power iaentical?

Firſt then, I will endeavour to "ſet Mr. H. right as to the identity of *animal exertion* and *mechanic power*:" and to this end it will be requiſite to anſwer the queſtion,—what is mechanic power? excluding, for the preſent, that acceptation of the term in which it is underſtood to denote one of the ſix ſimple machines. Now, it is pretty obvious, that the terms power, force, &c. when uſed in mechanical ſcience are purely metaphorical; for, as Profeſſor Dugald Stewart remarks, (*Elements of the Philoſophy of the Human Mind*, p. 202.) "All the languages which have hitherto exiſted in the world, have derived their origin from *popular uſe*; and their application to philoſophical purpoſes was altogether out of the view of thoſe men who firſt employed them." Language

Riſe and application of the terms, force, power, &c.

commenced amongſt ſimple men, who had little, if any acquaintance with what is now called ſcience: and in the gradual progreſs of moſt nations, from the ſavage to the ſhepherd ſtate, thence to the agricultural, and farther to the commercial ſtate, it would be very long before they would think of attaching any other meaning to the terms in the different languages, correſponding to power, force, action, reſiſtance, repulſion, &c. than thoſe which were manifeſtly referable to the different kinds of human, or of animal exertion: in ſubſequent times when ſcientific men began to claſſify, arrange, and ſyſtematize the phenomena which they obſerved in the congreſs, motion, and mutual operation of bodies, they found it much eaſier to denote the circumſtances they would deſcribe or treat of, by a figurative application of old terms, to which ſome analogous notions would neceſſarily be attached by every perſon, than to invent new ones, which would be attended by no ideas independant of an arbitrary definition. Nor, in this application, was there any danger of important

error

error, for the things to which the terms were appropriated would exhibit fuch fpecific differences as would almoft entirely preclude the chance of confounding one with another; and leave no room to fear, that when the terms were applied to inanimate beings, they would be concluded to exert ftrength, or to poffefs power, as animals did; any more than we fhould now fear being mifunderftood when we fpeak of the force of arguments, the attractions of benevolence, the fafcinations of beauty, or the repulfive tendency of envy. Thus, from contemplating the procefs of this gradual refinement, (a refinement produced not by barren fpeculators, but by the neceffary demands occafioned by the progrefs of civilization,) we fee that the words power and force, primarily ufed to denote animal energy, are now, by a natural extenfion grounded upon an obvious analogy, employed to exprefs *efficiency* in general. It will hence be eafy to affign the proper philofophical acceptation of thefe terms, when ufed in the fcience of mechanics. *Force or power, in a mechanical fenfe, is that, whatever it be which caufes a change in the ftate of a body, whether that ftate be reft or motion.* This definition does not require our entering into any metaphyfical difquifitions relative to the nature of caufes, or the connection of caufe and effect: that every event is brought about by fome caufe, that is, by fome agency, or fomething which precedes in the order of occurrence, is a truth which I think none will be difpofed to deny; but what is the agency, or where it actually refides, we can feldom know, except perhaps in the cafe of our own voluntary actions. It is not then the bufinefs of the mechanift, ftrictly fpeaking, to enquire into the *modus operandi*; we learn from univerfal experience, that the mufcular energy of animals, the operation of gravity, electricity, impact, preffure, &c. are fources of motion, or of modifications of motion; and hence, without pretending to know any thing of the effence of either of thefe, we do not hefitate to call them mechanical forces; becaufe it is incontrovertible that bodies expofed to the free action of either, are put into motion, or have the ftate of their motion changed. Forces therefore, being only known to us by their effects, can only be meafured by the effects they produce in like circumftances, whether thofe effects be creating, accelerating, retarding, deflecting, or preventing motions: and it is by comparing thefe effects,

Definition of the terms force and power.

Forces only known to us by their effects, thence meafureable.

or

or by referring them to some common measure of ready appreciation, that mechanics is made one of the mathematical sciences *.

Animal efforts are a *species* of mechanic power.

These observations will enable us to set Mr. H. "right as to the identity of *animal exertion* and *mechanic power*." Animal efforts are justly considered, both by the Mathematician and the practical Engineer, as constituting *one species* of mechanic power; when these efforts give to bodies equal momenta, or give to equal bodies equal velocities, it is truly said the animals exert equal forces; and we say that animal power is greater or less, as it is capable of imparting to bodies greater or less momenta, or as it is capable of stopping bodies moving with greater or less momenta: and the language of scientific men is analogous to this when they speak of any forces whatever.

Reply to Mr. H's remarks on a former statement.

It is now time to proceed to Mr. Hornblower's animadversions upon the instance I adduced (and many others might be adduced) to shew that Mr. Smeaton's measure of mechanical power and effect is not universally applicable. I asserted,

* That Mr. Hornblower may not rest upon the mere authority of any theoretic man, I beg to throw into this note an extract from the *Mechanique Philosophique* of M. Prony, an Engineer, who unites with a profound acquaintance with the theory, an extensive practice, and whose example in this respect I should sincerely rejoice to see more frequently imitated in this country.

"La nature de cette cause de mouvement, nommée *force* ou *puissance*; nous est tout-à-fait inconnue: l'homme appelle *force* la faculté organique qu'il a de se mouvoir, de s'arrêter, de produire ou de faire cesser le mouvement des corps qui l'environnent; et sans savoir en quoi consiste cette faculté, il a supposé qu'il existait quelque chose de semblable dans les agens physiques qui sont ou qu'il croit être, sur le globe terrestre et dans l'univers, les causes du mouvement de différens corps. Mais nous n'avons en mecanique, aucun besoin de connaître la nature de la *force* ou *puissance* qui est représentée, mesurée et introduite, dans le calcul, uniquement par les *effets* qu'elle produit. Ces effets se réduisent toujours à des vîtesses que les puissances ou tendent à donner, ou ont effectivement données à de certaines masses." "Parmi les diverses puissances que la nature nous offre, il en est une très remarquable dont il convient de prendre les effets pour terme de comparaison de ceux des autres puissances; c'est la *pesanteur terrestre* a la surface de la terre," &c. p. 20.

that

Reply to Mr. H's remarks on a former statement.

that a horse standing still and sustaining a weight which hung by a cord over a fixed pulley, would, after a due interval of time, be completely fatigued, although neither the animal nor the weight moved, and that, of consequence, there was a power expended, of which Mr. Smeaton's rule did not furnish an adequate measure. Mr. H. as though he understood me to affirm, that fatigue was the *only* indication of mechanical power expended, instead of limiting it to animal efforts as the connection evidently required, exclaims, "it is really difficult to be grave on this occasion:" p. 268. and argues in a kind of exulting strain which favours a little, I am afraid, of the spirit alluded to in the French proverb, *Chanter le triomphe avant la victoire!* Let us, says this gentleman, have a "*post* instead of the horse, and surely that will not tire, and what will be the consequence then? why then there will be no power expended, and no effect produced." Mr. H. then, it would seem, has forgotten that the post is retained in *its* situation by a force which in this case opposes that of gravity acting upon the suspended weight. The cord running over the pulley and sustaining the weight, being fastened to the post, would move it, were it not that the cohesive force of the earth in which the post is fixed, changes the state into which the post would be brought by the action of gravity upon the weight, and is sufficient to retain the whole at rest. If the post were set in loose sand, or in a quagmire, the weight would draw it away, and then I suppose, even according to Mr. H's notion, there would be a power expended, and an effect produced. So likewise, in Mr. H's other example, of the hat hung upon the pin, the force of gravity is balanced by the cohesive force of the wood or other matter, into which the pin is fixed. But it would be egregious trifling to dwell much longer upon such instances as these. Mr. H. conceives, if I have not completely misunderstood his meaning, that there is no "mechanical power" that is not "made up of a mass of matter moving with a determinate velocity;" and as such an opinion must either arise from neglecting to discriminate between cause and effect, or from a virtual denial of the whole doctrines of statics, (in which powers are excited without any motion being produced,) I shall hope to be excused though I waste no time on a refutation of any such position.

Indeed,

Indeed, much of Mr. H's reasoning, not only with respect to the post and the hat-peg, but throughout his paper, seems to rest upon a tacit admission (not a direct avowal, it is true,) of the erroneous notion, that forces exerted by animated beings, and those operating through the intervention of inanimate things, are totally distinct, and cannot be substituted the one for the other, or have a fair comparison instituted between them. Whereas, on the contrary, not only the theory but the practice of mechanics, proceeds upon the principle, that those forces are equal in degree, however different in their origin, or various in their mode of operation, which produce equal effects. Thus, for a familiar example, in the boring of a piece of ordnance; the borer may either be brought up to its proper position in the gun by the action of a man on the handles of a wheel, connected with the borer by rack and pinion work, or by the action of a weight attached to the farther end of a lever proceeding from the axle of the same wheel: and Mr. H. might as well deny the possibility of the work performed being the same in both these cases, as deny that a weight is kept from falling by an equal force, when prevented either by an animal, or by a fixed inanimate object; or deny that there is an expenditure of mechanic power when a man counteracts the operation of gravity upon his arm, when extended horizontally. While speaking of the proposition which includes any such denial, we may safely apply to it Mr. H's own language;—"A more erroneous proposition was never introduced into the theory or practice of mechanics."

Familiar illustration.

Mr. Hornblower has taken the trouble of extracting several passages from the Article MACHINERY, *Sup. Ency. Britan.* and among them has taken that which exhibits Professor Robison's measure of the exertion of a man, who walks at the rate of 60 feet per minute, and raises a weight of 30 pounds. The measure 57600, which this gentleman thinks enormously too large, is, in fact, too small, in so far as it does not include that part of the exertion required by the man to move himself. It was this omission of the learned Professor that induced me to lay down the general statement at p. 152 of your 43d Number, though I thought it might be deemed invidious if I specified my motives in that place. But when Mr. H. had commenced the labour of extracting, it would surely

Mistake of Professor Robison.

surely have been candid to produce a passage which strikes with great force against the universality of Mr. Smeaton's measure, at the same time that it admits the utility of this measure to engineers in many cases. This passage is as follows:—"When a weight of five pounds is employed to drag up a weight of three pounds, by means of a thread over a pulley, it descends with a motion uniformly accelerated, four feet in the first second. Mr. Smeaton would call this an expenditure of a mechanical power 20. The weight three pounds is raised four feet. Mr. Smeaton would call this a mechanical effect 12. Therefore the effect produced is not adequate to the power expended. But the fact is, that the pressure, strain, or mechanical power, really exerted in this experiment, is neither five nor three pounds; the five pound weight would have fallen 16 feet, but it falls only four. A force has therefore acted on it sufficient to make it describe 12 feet in a second, with a uniformly accelerated motion, for it has counteracted so much of its weight. The thread was strained with a force equal to $3\frac{3}{4}$ pounds, or $\frac{3}{4}$ of 5 pounds. In like manner, the three pound weight would have fallen 16 feet; but it was raised four feet. Here was a change precisely equal to the other. A force of $3\frac{3}{4}$ pounds acting on a mass whose matter is only three, will in a second, cause it to describe 20 feet with a uniformly accelerated motion. Now 5×12 and 3×20, give the same product 60. And thus we see, that *the quantity of motion extinguished or produced, and not the product of the weight and height, is the true unequivocal measure of mechanical power really expended, or the mechanical effect really produced; and that these two are always equal and opposite.* At the same time, Mr. Smeaton's theorem merits the attention of engineers; because it generally measures the opportunities that we have for procuring the exertion of power. In *some* sense, Mr. Smeaton may say, that the quantity of water multiplied by the height from which it descends in working our machines, is the measure of the power expended; because we must raise this quantity to the dam again, in order to have the same use of it. It is expended, but not employed, for the water at leaving the wheel is still able to do something."

Robison on Smeaton's measure.

In opposition to all this, Mr. H. I suppose, would say that this is not a case in point, because, "if the weight descends quickly

Messrs. Hornblower and Smeaton both concede the point in debate.

quickly it is sensibly compounded with another law, viz. the acceleration of gravity." Or, adopting other language of Mr. Smeaton, he might restrict his measures to "the height through which a body *slowly* and *equally* descended, or to which it was raised." But if, instead of the body's ascending or descending slowly and equally, it moved rapidly and irregularly; or, if the motion was reciprocating, the velocity increasing from quiescence to a certain magnitude, and diminishing to quiescence again; or, if we refer to the retarded rise and accelerated fall of heavy stampers; in such cases if Smeaton's measure be applicable, I wish to see its manner of application explained; and if it be not universally applicable, a point which is, in reality conceded both by Mr. H. and Mr. Smeaton, there is then as to this head no ground of difference between us, and Mr. H's last letter becomes in a great measure a superfluous labour; for, admitting the want of universality in the rule, is admitting all that I affirmed. Had not the measure been often very injudiciously exhibited as *universal*, a thing which Mr. Smeaton himself certainly never intended, I should not have at all referred to it in my former paper.

In correct language *weight* and *heaviness* ought to be distinguished.

It may be deemed a slight deviation from the immediate object of this letter, but I trust a justifiable one, if I briefly notice the surprize expressed by Mr. H. on account of Professor Robison's distinguishing between weight and heaviness. That the three terms *gravity*, *weight*, and *heaviness*, admit of a palpable and obvious distinction, is, in my opinion, indubitable: and till this time, I imagined it was universally reckoned one great excellence of an accurate philosophical disquisition, that it comprised a careful discrimination of the various acceptations of these and other terms, which were commonly reputed synonimous. There may, undoubtedly, be occasions in which a cautious selection from words of nearly similar import may be dispensed with: but there are many more, particularly when handling philosophical topics, where this careful choice cannot be safely neglected. And an attention to this point appears the more necessary, when it is recollected, that greater part of the controversies which have been agitated by men of science, have been rather verbal, than relative to things in themselves. To contend for the use of many terms to express one idea, instead of seeking for

for adequate separate expressions to denote every idea the mind can form, is to sacrifice precision and accuracy at the shrine of an ill-judged superfluity. The common resemblance between words esteemed synonimous, does not comprehend the aggregate signification, but some isulated particular attendant upon all, in some such manner as may be traced in individuals of the same species: there is generally one, if not more qualities, on which a manifest distinction depends; and the determination of such qualities is highly deserving the notice, not only of the linguist, but of all who aim at philosophical precision. I have not leisure to look attentively over twenty-five closely printed quarto pages, in order to find how Professor Robison distinguishes heaviness from weight. But the labour is unnecessary; for the distinction has often been made; and I will take the liberty of delineating it in the words of an author who is in no danger of having his sentiments warped to square with the tenets of any speculative mechanical system: I now advert to Dr. Trusler, who in his work on synonimous words speaks thus:— Trusler's remarks on heaviness and weight.

"*Heaviness, weight.*—In the figurative sense the difference of these words is so extremely great, as needs no pointing out; in the literal indeed, they are often confounded: considered then in this last sense, *heaviness* is that quality in a body which we feel, and distinguish by itself: *weight* is the measure and degree of that quality, which we cannot ascertain but by comparison.—We say absolutely, and in an undetermined sense, that a thing is *heavy*; but relatively, and in a manner determined, that it is of such a *weight*, for example, of two, three, or four pounds.—A thousand circumstances prove the *heaviness* of the air; and the mercury determines its exact *weight*." Vol. I. p. 133.

My letter has attained a much greater magnitude than was at first intended, and I will now conclude it. The remarks I have been tempted to offer, are founded upon the most correct interpretation I could put upon Mr. Hornblower's language; and if I have any where misunderstood his meaning, I shall be pleased to see that misunderstanding candidly removed. I entertain great respect for that gentleman's talents as a practical engineer; though I cannot but think him completely wrong in most of those remarks which have occasioned this communication. I have replied to such of his strictures

strictures as bore any relation to myself, I believe, without acrimony: but I have a deeply rooted aversion to every thing that wears the garb of controversy, and ardently hope the discussion on my part will be permitted to terminate here.

I am, Sir,

Your's, with much respect,

OLINTHUS GREGORY.

Royal Mil. Academy,
Woolwich, Aug. 9, 1805.

IV.

Description and Effects of an Apparatus for raising Water by Means of Air condensed in its Descent through an inverted Syphon. By Mr. WILLIAM CLOSE. *From the Inventor.*

To Mr. NICHOLSON.

SIR, *Dalton, July* 27, 1805.

Reference to the author's syphon engine.

IN one of my letters, some time ago, I briefly noticed an experiment I had made, to determine the practical value of the hydraulic machine, or inverted syphon, represented and described in the first volume of the present series of your Journal *, observing, that, at some future period, I might probably transmit to you a more particular account. Having since repeated the experiment, I now send you a letter upon the subject, for I am of opinion that a machine operating upon the principle, when constructed in the manner herein described, will answer very well, in certain situations, to raise water for domestic purposes; and although it may not be competent to perform half as much work as a bucket engine by a forcing pump, yet it may be kept continually employed, and be subject to very little wear, as its operation will almost be performed without friction.

Description of another apparatus;

The inverted syphon when applied to raise water in the manner described in this letter, has its higher orifice placed in a situation to receive both air and water at the same time. The air being conveyed by the velocity of the aqueous column

* See Philos. Journal, Vol. I. p. 30, Pl. IV.

to

to the lowest part of the syphon, and collected in a vessel, is employed as the medium for conveying pressure to raise water in another part of the apparatus.

In May 1803, I determined to find by experiment, under what degree of pressure it would be most advantageous to collect the condensed air, and likewise the proportion then existing between the two fluids moving in the syphon. The apparatus constructed for this purpose, is represented in *Plate I. Fig.* 1. It required only a small supply of water, but condensed the air sufficiently to be employed in the actual construction of a machine upon the principle.

being an inverted syphon, which carries down air and condenses it;

A round vertical pipe A B, half an inch in diameter, and 22 feet 5 inches in length, had its higher end placed in the cistern A, and its lower connected to a small oblong vessel C, which had an inverted glass bottle cemented upon a projecting cylinder on its upper side. From the other end of the vessel ascended another vertical pipe D E, half an inch in diameter, and 18 feet 3 inches in length, and terminated in a crook, 4 feet 2 inches below the highest part of the pipe A B.

The whole apparatus being filled with water, the cistern having a constant supply sufficient to keep the surface of the fluid just above the orifice of the pipe A B, when the orifice of emission at E was opened, the water flowing through A B, carried bubbles of air into the vessel C, which ascending, displaced the water in the bottle, and afterwards that contained in the vessel C, above the lower ends of the pipes A B and D E. At the first efflux, and after the descent of every material portion of air, the jet at E was projected several inches from the adjutage, but its curve decreased during the descent of more air; for the bubbles did not rise incessantly into the bottle, but after short intervals of rest, dislodging two or three ounces of water each time, with a guggling noise, which was very audible to the person regulating the supply of the cistern. After the water in the vessel C was depressed to a level with the ends of the pipes, the dense air carried down A B, ascended through D E, and caused frequent interruptions in the jet; for, expanding under a light pressure, it expelled the water in the highest part of the pipe with violence, and then the efflux ceased for some time after.

and this air is afterwards used for raising water.

The condensed air, however, could any time be let out, by a small pipe which was placed within the bottle, and opened on the outside of the vessel C.

The pipe A B had a joint above the bottom of the cistern, to facilitate the trial of mouth-pieces of various forms, to find by which the apparatus would sip the most air: and it appeared that no form, or position, conduced more to this effect, than when the pipe was crooked at top to receive the water in a horizontal current, and the higher side of its orifice was not more than two lines below the surface of the water in the cistern. It also appeared, that no less quantity of air was collected, when the diameter of the orifice of emission was reduced to four lines, than when it was half an inch.

Experiments to shew how much air can be carried down with he water.

After several experiments to determine the quantity of water requisite to supply the expenditure from the cistern, and keep the surface of the fluid accurately at the height best adapted to the operation of the apparatus; several trials were made to ascertain the quantity of air a given quantity of water would convey into the bottle in a given time. The results of several trials on the 21st of May 1803, were as follow:

1. The fall being 50 inches, and the orifice of emission four lines in diameter, the inverted bottle above C, holding ten ounce measures of water, was filled with air, under the pressure of a column 18 feet high, by 14 pints of water flowing out at the orifice of emission at E, in 145 seconds. 2. By 13 pints, in 133 seconds. 3. By 12½ pints, in 125 seconds. By 12 pints. 5. By 11 pits, in 95 seconds. 6. By 14 pints, in 114 seconds. 7. By 12 pints, in 102 seconds. 8. By 12 pints, in 108 seconds.

9. The orifice of emission being half an inch in diameter, the bottle was filled by 12 pints. 10. By 13 pints, in 133 seconds. 11. By 12¼ pints.

12, 13. The fall being 44 inches, the orifice of emission four lines in diameter, 11 pints filled the bottle in 95 seconds; and 14 pints, in 120 seconds.

With an height of 18 feet and fall 50 inches, 20 parts of water carried down one of condensed air.

The difference in the time, and the quantity of effluent water required to fill the bottle with air, in these trials, was probably occasioned by a portion of the air being sometimes contained in the higher, and at other times in the lower part of the pipe A B, at the commencement of the effusion: or, perhaps,

perhaps, in part, by the water in the cistern not being always of the same height; for the cistern did not overflow, but was supplied with great care, sometimes by a pump, and sometimes by lading water out of a vessel, always keeping the supply from agitating the contents of the cistern as much as possible. Had the bottle been larger, there had probably been more uniformity in the results of the trials. In estimation, I think, however, we shall not overrate the operation of this machine, by taking 15 pints for the mean quantity of effluent water emitted while the bottle was filling with air; and then deducting the quantity expelled from the bottle, it will appear that 20 parts of water carried one of air down the pipe A B: and as one ounce measure of condensed air at least was collected in 14 seconds, so 16 pints would collect every hour.

Some few days after these experiments, the pipe A B was lengthened to 24 feet 7 inches, and D E to 20 feet; but upon trial, the air was carried into the bottle so much slower than before, that a suspicion arose that some part of the apparatus was not air-tight; and on this supposition the pipes were taken down. Trial with a greater length of syphon:

In February 1804, the pipes, &c. were examined, and set up again with considerable care. A B was 24 feet 7 inches long; D E, 21 feet one inch; consequently the difference for the fall was 3 feet 6 inches. With this apparatus, when the diameter of the higher orifice of the pipe D E was four lines, it appeared by four trials (Feb. 25, 1804), that the bottle lost only one ounce of water per minute. Less air was carried down.

When the pipe D E was shortened to 19 feet 7 inches, and had its higher orifice five feet below the surface of the water in the cistern, four ounce measures of condensed air descended into the bottle, during the emission of 16 pints of water, through the orifice at E, when half an inch in diameter.

The diminution in the collection of air, in these last experiments, was much more considerable than was expected to happen, either from the absorption of the water, or the increased condensation of the air, which might be occasioned by so small an addition being made to the apparatus. The jet at F was projected more steadily in these last, than in the preceding trials; and the condensed air, instead of rising into the bottle in large detached bubbles, ascended in a continual stream, like the evolution of gas from the bottom of an effervescing

 mixture.

mixture. From the minute division of the air, it is not improbable, that a small portion might be carried along with the current of water under the bottle, and ascend through the pipe D E; but this was not determined. If there was no defect in the apparatus, it appears, that it will not be so advantageous, in the construction of a working machine upon this principle, to employ a condensing column so heavy as 24 feet, as one that is lighter.

Other trials.

Feb. 28, 1804. The pipe A B being shortened to 22 feet 5 inches, and D E to 18 feet 3 inches, the orifice of the adjutage at E being four lines in diameter, the bottle was emptied by 13, 12½, 14, 13, 13, 12, and 14 pints of effluent water, in seven successive trials, as in those of May, 1803.

March 9. With a fall 3 feet 9 inches, A B being 15 feet one inch, half a pint of air was collected during the discharge of 12½ pints of water. Again, the orifice of emission being four lines in diameter; the fall 4 feet 2 inches; A B 15 feet 8 inches; D E 11 feet 6 inches; nine ounce measures of air were conveyed into the bottle, in one minute, during the discharge of 10 pints of water, in five successive trials: and when the diameter of the higher orifice of the pipe D F was half an inch, the same quantity of air was carried down in 50 seconds, by nine pints of effluent water, including that displaced from the bottle. When A B was 15 feet 8 inches; D E 13 feet 2 inches; the fall 2 feet 6 inches; nine ounce measures of air were collected in the bottle, by the discharge of 16 pints, in 90 seconds; in 105 seconds, by 18 pints; and again, by 18 pints, in 90 seconds.

March 16. The fall being 2 feet; A B 8 feet 5 inches; D E 6 feet 5 inches; and the diameter of its higher orifice half an inch; ten ounce measures of air were collected in one minute; again in 64 seconds, when the effluent water measured 10 pints; and again in one minute, when 10 pints.

Having now shewn what power a machine operating upon this principle may be expected to possess, I proceed to shew how its principle may be applied to practice.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

Fig. 2, *Plate I.* exhibits a machine for raising water above the cistern.

R represents a cistern supplied by a spring, where there are four or five feet fall for the water.

W W,

W W, a well or pit ſituated below the bed or lower level of the ſtreamlet; its depth varying from 6 to 20 feet, according to the elevation to which water is to be raiſed above the ciſtern, and the number of progreſſive columns by which it is to aſcend.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

A A, a pipe leading from the ciſtern R, to a bell-ſhaped veſſel B, fixed a little above the bottom of the well, with its mouth downwards. The top of the pipe is crooked, as repreſented at A, *Fig.* 5, and there is a joint below, which allows the crooked part to be detached from the reſt. The lower end of this pipe is alſo crooked, and turns under the ſide of the veſſel B.

C C, a pipe fixed into the top of the veſſel B, and carried a little above the ciſtern, where two ſmaller pipes, E and G, are connected with it by a ſtop-cock.

E, a ſmall pipe leading to F, a veſſel or chamber, placed in the ciſtern R.

G, another ſmall pipe leading to H, a veſſel or chamber, ſomewhat leſs than F, placed in a higher ſituation. This pipe has a turn, a foot above the top of the veſſel H.

I, a pipe leading from the ciſtern R to the veſſel F.

K K, a pipe deſcending a foot or more below the veſſel F, and then aſcending to the veſſel H.

L, a pipe connected to K, a foot below H, and then carried to the conduit or ciſtern which receives the raiſed water.

The pipes I, K, L, have each a valve opening upwards.

The conſtruction of the cock is repreſented in *Fig.* 3. The conical barrel has four holes, C, E, G, O, and the turning part or key, has a notch, or hollow, on each ſide in that part which moves oppoſite thoſe holes, ſo that the pipes C and E, or C and G, may be connected by a quarter of a turn. When the communication opens between C and E, the external air has acceſs to the inſide of the pipe G, and to the chamber H above, through the opening O; and when C is joined to G, the air is admitted into the chamber F, through the opening O, and the pipe E.

In the narroweſt part of the veſſel B, a buoy or float is fixed upon the elevated end of a crooked lever, moving upon a horizontal axis, ſupported by pieces attached to the ſide of the veſſel. Inſtead, however, of a common hollow buoy, it will be preferable to uſe a body ſpecifically heavier than water, and by

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

by adding weight to the other end of the lever, to make such adjustment, that while both are immersed in water, the body within the vessel B, shall be a few ounces lighter; and, on the contrary, when it alone is above the water, it shall be so much heavier than its counterpoise, which is covered. A small cylindrical vessel $2\frac{1}{2}$ inches in diameter, and the same in depth, filled with water and closed, will probably be size sufficient for such a float; and the proper counterpoise may be very readily and easily found, by having the lever fixed on its axis in a vessel of water, repeatedly drawing off and replacing the fluid in that part of the vessel which contains the float; and increasing or diminishing the weight, until the proper adjustment is obtained. But to proceed with our description.

S represents a small syphon suspended by a lever, with one branch in the inside, and the other on the outside of the cistern R. The outside branch being re-curved in the manner represented in *Fig.* 5, it is evident, that when the instrument is filled, it will draw water out of the cistern whenever the orifice of the re-curved branch is depressed below the level of the water in the cistern; and that its operation will be suspended by raising the same a very little above that level. From the contrary end of the lever, a chain or wire descends, and is connected to the lever which carries the float; and by this connection, the syphon is suspended with the orifice of its re-curved branch above the surface of the water in the cistern, while the float occupies the highest part of the vessel B; for the weight of the syphon and that of the included column must be so nearly counterpoised by the chain and an additional weight, that it cannot depress the float, though it must possess sufficient weight to descend when allowed by the descent of the float.

M represents two cuneiform buckets, connected at their bases by a transverse partition, and fixed upon a horizontal axis, as is more clearly exhibited by the section, *Fig.* 4. When the bottoms of these are placed in an oblique direction, making an angle with the horizon of 25 or 30 degrees, as represented in the drawing, (*Fig.* 2.) and a small stream of water falls from the syphon, the higher bucket will receive the water, and falling in consequence of its load, will raise the other bucket, which will now receive the water, and by falling will raise the first, whose contents were emptied in its descent.

descent. A small arm or lever is fixed to the axis of this apparatus, and connected by a piece of strong wire to another shorter lever fixed upon the smaller end of the turning part of the cock C, and by this means the cock moves with the alternate motion of the buckets M, when supplied with water.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

When the space from the top of the vessel B, to the surface of the well is equal to 18 feet, the top of the chamber H, above the bottom of F, and the perpendicular height of the pipe L, above the bottom of the chamber K, may be each 18 feet.

The valves and every other part of the apparatus being in proper order for work; the well being filled with water; and the reservoir R constantly supplied, so as to overflow in one part somewhat lower than the rest of the brim, while the higher orifice of the pipe A A, is about two lines below the surface of the water, and takes in its full quantity; the manner of bringing this machine into action, and its operation afterwards, may be understood by attending to the following directions, and statement of particulars:

Open the pipe E which leads to the chamber F, by turning the cock C, and water will descend into F from the cistern R, by the pipe I: When the chamber is full, place the two connected buckets M, in a horizontal position, and the cock C, if properly constructed and connected with these buckets, will cut off all communication between the pipe C C and the pipes E G. The air carried down the pipe A A, by the column of water which descends and keeps the well constantly overflowing, will ascend into, and gradually expel a quantity of water from the pipe C C, and afterwards that contained in the higher part of the vessel B also. The float receiving an accession of weight by being out of water, will descend and let down the re-curved syphon S, which will pour water upon the buckets below. At this period depress that bucket which by descending opens a communication between the pipes C and E, and no farther attendance will be requisite.

The pressure of the column in the well, above 18 feet high, being thrown upon the water in the chamber F, by the intervention of the condensed air in the pipe C, the valve in the pipe I will be shut, and water will ascend from the chamber F through the pipe K K into H; the syphon S will also ascend into its place, and cease to draw water from the cistern.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

cistern. An equilibrium being soon established between the acting column in the well, and the re-acting one included in the pipe K K and the chambers F, H, by the expulsion of the air from B, the water will afterwards ascend much slower into H than at first, an equal depression of the fluid being produced in the pipe C and the chamber F, by the collecting of the condensed air.

In more or less time, according to the capacities of the chambers, so much water will be expelled from the lower as will fill the higher, the air having been expelled from this last, through the pipe G, whose outlet is at O in the cock C E G O, *Fig*. 3. Water will then begin to ascend into that part of the pipe G which rises from the top of the vessel, and the acting column being lengthened in proportion to the increasing height of that which it counterpoises, the condensed air will depress the water in the vessel B below the float, which descending, will lower the re-curved syphon, and water will fall into the elevated bucket M, which soon afterwards in consequence of its load will descend, and by moving the cock above, will open a communication between the pipes C and G, and between the inside of the chamber F and the external air, when the condensed air will rush out, and this vessel refill with water from the cistern.

The force of the acting column being now thrown upon the contents of the higher chamber H, the valve in the pipe K will close, and water will ascend through the pipe L, into the cistern appropriated for its reception.

At the first opening of the communication between B and H, the re-curved syphon S will pour water into the bucket last elevated; but before the load is sufficient to move the apparatus, the syphon, if properly adapted to its purpose, will be drawn up again, in consequence of the condensed air being expelled faster from the vessel B, than it descends by the pipe A A: for the water will always rise with the greatest rapidity after the turning of the cock, because of the difference subsisting between the acting and re-acting columns, and the air previously stored up in the vessel B. The supply of condensed air, however, being inadequate to support the difference, an equilibrium soon takes place, by the water ascending into the lowest part of the pipe C.

The

The supply of condensed air will now continue to force water out of the chamber H, and to depress the fluid in the pipe C, in an equal degree, until all the water in the chamber is expelled. The water will then sink in the highest part of the pipe K, and the acting and re-acting columns proportionally lengthening, air will collect in the vessel B; the float and re-curved syphon will descend; and before the depression of water in the pipe K reaches the lower end of the pipe L; before the condensed air can escape by expelling the water contained in this pipe, the elevated bucket M will fall in consequence of water poured into it by the syphon, and the communication between C and E being opened, the condensed air will rush out of H, through the open pipe G, whose outlet is at O in the cock C E G O, *Fig.* 3; the valve in L will support the water above it, and the force of the acting column being again thrown upon the water in the chamber F, the fluid will begin to ascend into K; the re-curved syphon will rise and continue in its place as before, until the water begins to fill that part of the pipe G which rises from the top of the chamber H, or, if there cannot be a sufficient quantity raised from the lower chamber to fill the higher, until it begins to be depressed into that part of the pipe K, connected with the bottom of the chamber F; the syphon will then be let down by the float, will pour water into the bucket last elevated, and thus again open the communication between the air-holder B C, and the higher chamber, from which the water will be expelled; and in this manner the alternations will proceed, the machine continually raising water from one or other of its chambers.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

By additional pipes and chambers, similar to G H L, a smaller quantity of water may be raised still higher, the lowest additional chamber being fixed upon the top of the pipe L, and the pipe for supplying it with condensed air, connected to the pipe E. A pipe for supplying a chamber still higher must be attached to the highest part of the pipe G; and it is evident, that if E leads to two chambers and G only to one, the machine will regulate its operations so as to lose no time. It will be requisite, however, to fill the additional chambers with water before the machine is set to work.

Where the supply of water is variable, the machine may be adapted by having several pipes similar to A A, but some wider

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

wider and others smaller, and by setting such of these to work as are requisite, for whatever may be the increase or diminution of power, the turning of the cock will be duly regulated by the float.

The water which supplies the pipe I, and ascends in the apparatus, should be cleared, by filtration, from impurities and substances that would obstruct the closure of the valves. The cistern should have a moveable piece at the place where the water overflows, to accommodate the surface of the fluid to the ends of the pipes, that the full quantity of air may descend with each column, and that the maximum of effect may be obtained from the supply.

To determine how well such a combination as that I have described would answer the purpose, I had a model constructed upon the plan exhibited in *Plate I. Fig.* 2; but having no convenience for an overflowing well or cistern, I was obliged to modify some parts, in a manner tending to diminish its power.

The water from the cistern R, falls into a capacious vessel, from whence, when the machine is at work, a hand-pump continually raises it again into a vessel above, which supplies the cistern R through a pipe nearly half an inch in diameter, under a constant pressure of 3½ inches charge. The supply keeps the cistern continually overflowing, and the surface of the water is calm and always at the same height.

The pipe A A is 8 feet 3 inches in length, and half an inch in diameter. Its lower end is inserted into the vessel B, which is closed at bottom, and constructed of such a form as to include the lever which carries the float. Above that end of the lever bearing the counterpoising weight, a vertical pipe 6 feet 3 inches, is connected to the top of the vessel; and through this pipe, which is no wider than A, the water ascends and flows to the pump: A chain, consisting of pieces of wire four or five inches in length, looped together by the ends, also passes through it, and connects the float-lever to that which moves the re-curved syphon S.

The bottoms of the buckets M are both together 14 inches long and 6 broad. The base partition is 4 inches high. Each bucket has an end parallel with the base, one inch deep, provided with a hole to let out the water when depressed. The cock moves with less than a pint of water in the elevated bucket.

The

The float in the vessel B, is a small cylindrical copper vessel, one inch in height and two inches in diameter. It was filled with water and closed before it was fixed in its place. Though equal in bulk only to one ounce and three quarters of water, yet it is quite sufficient to move the syphon which would work a larger machine.

Description of the machine as constructed for raising water, by air condensed in an inverted syphon.

The pipe C is half an inch in diameter: E, K, G, L, are smaller. The valves are leather.

This machine, when in good order, raises water nearly 12 feet above the cistern, at the rate of $20\frac{1}{2}$ pints per hour, and performs all its operations as well as can be desired. When first set to work, the cock is so placed as to close the top of the pipe C, until the condensed air begins to collect in the vessel B, and then the communication is opened to the chamber F. If the communication was open at first, the water would be expelled from F into the cistern, while the pressure was insufficient to close the valve in the pipe I.

The chambers F H being small, the syphon moves frequently; but in a working machine these vessels should not only be broad and shallow, but capacious, that the wear of machinery may be reduced to its utmost extent.

To determine what quantity of water flows through the apparatus, I fixed a spout upon the top of the ascending water pipe; but in doing this I entangled the float in the vessel B, that it could not be made to work the syphon. The effluent water, in this unemployed state of the machine, including half a pint displaced from the vessel B, amounted to $8\frac{1}{2}$ pints. The superfluous water from the cistern R measured $8\frac{1}{2}$ pints also. If the machine had been working the waste water would have been less, as part would have been drawn off by the syphon.

From several trials, this model appears to raise water above the rate that might be estimated by the experiment of March 6, previously related.

In actual practice, I think the allowance for waste and working the buckets, of one third or perhaps only of one fourth of the supply, will be sufficient; then supposing the apparatus so adapted to the supply, as 28 or 30 gallons from the cistern will raise one gallon 18 feet, so 84 or 90 gallons will raise one gallon 44 feet, by three ascending columns.

The

The bucket-engine at Irton-Hall, in Cumberland *, is faid to raife one gallon of water 60 feet high by 36 gallons fupply; hence, if the wafte water be included, it appears that our machine will not be competent to perform half as much work by the fame fupply, and its peculiar advantages muft depend upon its durability when conftantly employed.

I am, Sir,

Your's refpectfully,

WILLIAM CLOSE.

V.

Remarks on COUNT RUMFORD'S *Experiments relating to the Maximum Denfity of Water. In a Letter from Mr.* JOHN DALTON.

To Mr. NICHOLSON.

SIR,

Count Rumford's experiments on the max. denfity of water confidered.

IN your laft Number, page 225, is an interefting article on the queftion, At what point of temperature water is of greateft denfity? From the introductory paragraph I was led to expect, that all the material objections to the current doctrine were confidered and obviated, and that new and convincing arguments in its fupport would be adduced. In the former of thefe expectations I was altogether difappointed; and though the new experiments are ingenious and well worth attention, they are not quite fo demonftrative to me as they appear to be to Count Rumford. Perhaps we may both be too ftrongly biaffed towards preconceived theories: however this may be, it feems proper that when new facts are brought forward, we ought to reconcile them to the theory efpoufed.

Mr. Dalton's exper. in this Journal on the fame fubject.

At page 93 of Vol. X. of this Journal, I have ftated a number of facts and experiments which appear to me irreconcileable with the notion of water being denfeft at 40°. I believe it is denfeft at 32°, or the freezing point; and it is my prefent intention to fhew how, on my hypothefis, I explain Count R.'s refults.

* See Philof. Journal, Vol. II. p. 60.

Water

Water expands by heat from some point (whatever it may prove to be) by a law which is nearly that of the square of the temperature from the said point, as is evident from Sir Charles Blagden's table. Consequently the force of ascent, which water acquires by temperature, is at first very small, but increases to a very considerable amount before ebullition. The cohesion of the particles of water is a constant force; there will therefore be a point of equilibrium between these two forces; that is, a point at which the increased temperature will be but just sufficient to counteract the tenacity, in which case no internal motion can ensue. Whether a diminution in density in water to the amount of *one hundredth* or *one thousandth*, or *one ten thousandth* or more, is the point alluded to, is to be determined only by experiment. I apprehend that water of 40° is about one *ten thousandth* part lighter than water of 32°; but that this force of ascent is but just sufficient to counteract the tenacity, and consequently no motion takes place; in such case the diffusion of heat through water is the same as through a solid body. Whenever the difference in density exceeds that just mentioned, internal motion is the consequence, and that greater in proportion to the difference of density, which we know may amount to $\frac{1}{24}$ of the whole.

Count Rumford's experiments explained, on Mr. Dalton's hypothesis. Water has very little change of dimensions by heat or cold near its max. density.

This change at 40° is not sufficient to produce a current;

Count R.'s experiments therefore will be explained by observing that the thermometer acquired heat by the proper conducting power of water, as if it had been metal, or any other solid body; the temperature acquired was greater in the 2d experiment than in the 1st, because the heat of the ball was greater; but in the 3d experiment the heat of the ball was such as to produce a current upwards that almost precluded the descent of heat, by carrying away the heated particles as soon as formed.

—and therefore the heat at first passed downwards as if thro' a solid; but in higher temperatures there was a current, which prevented the descent.

The circumstances of the two thermometers by the side of the ball and the cup, in the two first experiments not acquiring any temperature, is certainly remarkable, and not easy to be explained, even upon Count R.'s principle; for, the supposed descending current of warm water should have filled the cup and overflowed, so as to affect the collateral thermometer.

Difficulty respecting the two thermometers.

One most important experiment Count R. has omitted, and which it is particularly desirable that he, or some one in possession of a similar apparatus, would perform, especially as it would go further than any other to establish the doctrine of *cur-*

Important experiment proposed. Let the water be at 40° and the solid at 32°.

rents

rents in water, when the temperature varies from 32 to 40°. This is to repeat the 1st experiment, with the difference that the mass of water should be at the temperature 40°, and the ball at 32°; in which case the thermometer in the cup would not be at all affected upon Count R.'s principle; but if the explanation I have attempted above be accurate, no material difference in the results of the two experiments would be observed.

I am yours,

J. DALTON.

Manchester, August 17, 1805.

VI.

On the Art of bending Wood. By J. H. HASSENFRATZ.*

Either live or dead wood may be bended.

THE operation of bending may be performed either on live or dead wood, the processes differing however for these two states.

I. *On the bending of Live Wood.*

Live wood may be

Live wood has a natural elasticity †, which varies according to its species, size, and age. The larger and older the wood, the less elasticity it possesses.

This operation is performed on wood when growing, either to straighten it, to give it a figure suitable to the ornamental purpose for which it is designed, or to shape it to the use for which the timber is intended when cut. Thus trees may be bended, which are intended for the building of ships, or for making the felloes of wheels in one piece.

—bended for ship-building, or for felloes of wheels, &c.

By fastening down young trees.

When trees are yet young and pliable, their stems are fastened down by ropes, or poles, or stakes, or frames. In this situation they are confined till they will retain when let loose the curvature that has been given them.

This the most easy process;

Of all the modes of bending timber the most easy and commodious is that applied to young growing trees: for their pli-

* Translated from the Journal des Mines, N. 94, p. 475, July, 1804.

† In many parts of this paper the writer seems to have used the word elasticité for the property of undergoing flexure without breaking. T.

antness

suppleness and elasticity enable them to acquire any form that may be desired; so that there are few to which the most whimsical figures may not be given, with due care and the requisite precautions; but at the same time we injure their natural constitution, retard their progress, and frequently reduce them to a state of constraint and disease prejudicial to their growth.

but injurious to the timber.

II. *Of the bending of Dead Wood.*

The bending of wood that is cut down and dead, though more difficult, is yet more in use, because we may choose such as is best adapted to the purpose for which it is designed, and then give it the suitable curvature.

The bending of dead wood is most advantageous.

The process generally employed is founded on the property caloric possesses of augmenting the elasticity of wood by penetrating it, and diminishing this elasticity on quitting it.

The principle is the action of heat, which increases the pliableness of timber.

Thus when we wish to bend thin pieces, as the staves of barrels, or the planks of boats, we heat them at the part that is to be curved, and bend them gradually as they grow hot.

But heat applied to one part of the wood, while the other is in contact with the air, heats it unequally, and increases the pliableness but partially; so that on bending it, some parts are stiff and others yield, occasioning an unequal curvature, and sometimes cracks or splinters in the inside or on the surface of the wood. The only method of remedying this inequality is to heat the wood equally throughout.

Partial heat affects the wood unequally, and occasions it to crack or splinter.

Furnaces or stoves gradually heated are adapted to the purpose of affording a uniform heat, and consequently facilitating the curvature of the wood; but in using them there is reason to fear, that the caloric, while heating the wood, may expel from it the fluids contained in it, char it, and wholly destroy its elasticity.

Furnaces or stoves heat it uniformly, but may scorch it.

The pliableness of wood is in proportion not to its temperature alone, but to its humidity likewise. The same wood at the same temperature will be more or less pliable in proportion to the water contained in it; and at an equal degree of moisture its elasticity will be proportional to its temperature.

Humidity as well as heat necessary to render wood pliable.

We have an instance of the double influence of heat and moisture in joining two pieces of wood with a tenon and mortise, where the mortise is only a third of the breadth of the piece that is driven into it to form the joint. These joints, so extraordinary in appearance, surprise people so much, that

The singular structure of a mortise and tenon.

Heat and moisture are applied to bend timber; most of those who use them make a mystery of them. The process employed in this operation has given rise to the method at present in use for bending with ease the largest and stiffest timber: it consists in penetrating it with humidity, and at the same time imparting to it a uniform temperature, then bending it, and letting it cool, while it is kept in the form to which it has been brought.

—In three different ways: For heating and moistening the timber, three different processes have been employed: first, boiling water; secondly, steam; the third, wet sand heated.

1st. By boiling water. The stove for the first process consists of a large copper boiler, heated by three furnaces, closed by a movable cover, and varying in its dimensions according to the size of the timber for which it is intended. Cranes are used for raising the timber, and putting it in or taking it out of the boiler, which is kept full of water. When the timber is in, the cover is put on and beaten down close, to diminish the evaporation of the water; the three fires make the water boil, the timber is heated and penetrated with moisture, and it is then taken out to be bent.

This dissolves some of the component parts, and lessens the dimensions of the wood. This process, one of the first that was employed, has the defect of dissolving a part of the proper substance of the wood in the boiling water; the timber shrinks in drying, so as to become narrower and shorter; its strength and elasticity are considerably diminished, and from these alterations occasioned by it the process is disused.

2d. By steam. Description of the steamer. Figures 2, 3, and 4, Pl. II. represent the plan and elevations of a steamer. It consists of a large wooden box, formed of stout planks, held firmly together by square frames. Within are supports for the timber that is to be exposed to the action of the steam. The dimensions of the box are regulated by the size and quantity of the wood intended to be softened.

For small steamers a boiler is fixed at one extremity of the wooden box, and the wood is introduced at the other through an opening, the door of which either slides in a groove or turns on hinges. For large ones the boiler is fixed in the centre, and there is an opening for the timber at each end. In the side opposite the boilers are openings *a a a* for arranging the timber on the supports. It should be covered with some bad conductor of heat. It is usual to leave the wooden boxes exposed to the air externally; but it would be of advantage to cover

cover the planks with some substance that is a bad conductor of heat, to confine the heat that is disengaged from the steam within.

Each boiler having a communication with the interior of the box, by means of a pipe, the steam is distributed to each stage by the tubes *b b b*, *Fig*. 3. The vapour arising from the boiling water penetrates the timber with moisture, heats it, increases its elasticity, and renders it fit to be bent.

Steamers require little care, and little expense, but they cannot be used for timber of any great thickness, since they cannot impart a emperature higher than that of boiling water, and this is not sufficient to give large pieces the degree of pliableness necessary for bending them. This process is easy in use, and not expensive; but it is not hot enough for large timber.

This lowness of temperature gave rise to the invention of the sand-stove, which is formed of four stone or brick walls. In the middle are two furnaces, with which several circular flues communicate, for conveying the heat, the heated air, and the smoke, to a chimney rising from each end. On these flues are plates of cast iron, which form the bottom of the cavity in which the sand is placed; the flame and smoke circulating in the flues heat these plates, and these plates heat the sand. This is an imitation of those sand-baths which have been long employed in many chemical processes and in several manufactories. 3d. By wet sand. Sand-stove described.

As the sand may be heated to a temperature above that of boiling water, it can communicate a greater heat to the timber; but were there nothing but sand and timber in the stove, all the gasefiable substances in the timber might be expelled by the heat, and the timber charred.

To prevent this, one or two boilers filled with water are placed in the middle of the stove. The water converted into steam by boiling penetrates the sand with moisture; this imparts moisture to the timber; and thus the heat that pervades the timber expels from it no more moisture than is replaced by the sand, so that all the proper substances of the timber are preserved. Steam must be used with the sand heat;

We will not venture to affirm however, that no portion of the component parts of the wood is evaporated in this operation, and that consequently it undergoes no alteration; but with the precaution of taking out the wood to bend it as soon as it is sufficiently heated and penetrated with moisture, the injury is imperceptible. and the timber will not be perceptibly injured.

 The

The sand-stove is covered throughout its whole length, to retard the evaporation of the moisture contained in it, and allow the heat to accumulate sufficiently to give the wood the proper temperature.

Manipulation for the heat.

The pieces of timber are introduced at the ends, placed in the middle of the stove in the direction of its length on bars fixed for the purpose, and covered with sand.

When the timber has been heated and penetrated with moisture to the proper degree for enabling it to assume the degree of curvature required, it is bent to a line designating the curve.

The wood may be bended horizontally or vertically,

The timber may be bent in two ways, either horizontally or vertically; the former is used for pieces of smaller dimensions and greater curvature.

—by any mechanic power;

In either way the force that produces the curve is applied by means of ropes, tackles, or even capstans. The piece must be kept in the shape to which it is brought, and thus left to dry and grow cold, when it will retain the curvature given to it.

--which may be applied various ways.

Frequently when the piece of wood is thin, pressure by hand, or by weights, will bend it sufficiently, so that it will retain its shape on cooling. But the means of bending it may be varied to infinity, according to the elasticity of the timber, its size, its temperature, and its humidity.

VII.

Experiments made in the great, in a reverberating Furnace on Cast Iron, confirming the established Theory respecting the Difference between cast and malleable Iron. By G. A. Lampadius, *Prof. of Chemistry and Metallurgy at Freyberg.**

The reverberatory furnace described.

I SHALL first describe the reverberatory furnace used in these experiments. It had three principal parts; 1. the air tunnel

* Extracted by J. F. Daubrusson, in the J. des Mines, from the *Sammlung Practisch-chemischer Abhandlungen* 'Practical Chemical Essays,' of Lampadius, Vol. II. p. 145.

Prize question of Bohemian Society, 1795.

In 1795, the Royal Society of Bohemia proposed as a prize question to settle the theory of the refining of iron, taking as a basis the labours of Vandermonde, Berthollet, and Monge, on the different states

tunnel and ash-hole; 2. the fire-place; 3. the hearth and chimney. To obtain the proper degree of heat, the air was conducted through a vertical tunnel several ells long (the Saxon ell is near two English feet), the lower aperture of which was over a stream of water, and consequently it brought rapidly to the fire-place a supply of fresh and condensed air. The fuel was wood; the bottom of the furnace was an oval cavity, covered with a heavy coating, and capable of containing three or four hundred weight of metal. The flame, which traversed the furnace with rapidity, escaped afterwards through a chimney eight ells high. The furnace had an opening capable of being closed at pleasure by an iron door. There was another above the fire-place, a few inches square, serving to admit the nozzle of a pair of bellows, or the neck of a retort.

It was an air furnace having an hearth within and its chimney was 16 feet high.

In the use we made of this furnace I had an opportunity of observing very distinctly, that in the flame of a closed reverberatory furnace there are always a multitude of unoxided particles of carbon, which impart to it the capability of reducing (disoxiding) metal. This opinion I had already announced on occasion of a memoir of Mr. Dacamdra. In some of our trials, making use of the wood of the Scotch fir, we observed, that the smoke issuing out was black and dense, and this the more the fresher the wood; but as soon as we made use of the bellows, the flame appeared clear, because the oxigen introduced by the air or vapour oxided the carbon that was in the flame, and thus produced a greater heat.

Unoxided particles of carbon visible in the flame of this closed furnace.

First Experiment with the simple Fire of the Furnace.

Exp. I.

The furnace having been heated for some hours, and the fire being very violent, about three hundred weight of metal was taken from the crucible of the high furnace, and poured into the reverberating furnace. This cast iron, when become solid, was gray, and of a fine grain. At the expiration of an hour a frothy scoria appeared on the surface of the metal, which, to

Gray fine grained cast iron in the reverberatory furnace, covered with frothy scoria, chiefly carburet of iron.

states of iron. Mr. Lampadius shared the prize. His memoir may be considered in general as a confirmation and supplement to the labours of the French academicans; the experiments which he made at Muckenberg in Saxony, in the iron works of Count Von Einsiedel, affording him fresh proofs of this theory. These experiments are here presented to the reader. D.

judge from appearances, consisted chiefly in carburet of iron. We attempted to remove it; but as some of the metal adhered to it, and came away at the same time, we desisted. Soon after, the furnace being closed, we heard a continual boiling, resembling that of a viscous substance in a close vessel. On opening the furnace, we perceived that the whole matter in reality boiled, and that bubbles were continually rising, which burst on the surface with a beautiful bluish flame. These jets of flame had the colour exhibited by carburated hidrogen gas. The boiling continued as long as the fire was kept up; at the same time a pretty large quantity of scoria was formed, which however could not be removed, on account of the viscous consistence now acquired by the metal. Besides, as the metal was frequently stirred to present a fresh surface to the air, the scoria mingled with it. At the end of five hours it was no longer fluid, and appeared to be refined. It had lost its gray colour and fineness of grain, was white and coarse grained, and showed itself more malleable, though it was not capable of being forged. The refiner carried it to his ordinary furnace, and there it was refined in less time, and required less labour than common cast iron.

Not removable for adhering metal.

The metal was brought to ebullition.

Carburated hidrogen gas evolved.

In five hours it became white and coarse grained, and a little malleable.

This was afterwards refined sooner than common cast iron.

As in this trial we were unable to separate the scoria, and no change had been made in the form of the hearth of the common refinery, which ought perhaps to have been done, nothing positive can be advanced with respect to the practical advantage of refining by the help of reverberatory furnaces; we were merely convinced of its possibility, and enabled to demonstrate the theory of this process, that is to say, of seeing clearly what passed in the operation. The cast iron was here converted into malleable by means of the oxigen that was in the little atmospheric air, which, jointly with azote and carbonic acid gas, covered the metal in fusion. This oxigen combined with the carburet of iron, and then carbonic acid gas and oxide of iron were formed; this produced the bubbles of air and the scoria. The lightness of the frothy scoria, which arose to the surface at the beginning, was the reason of their separation from the rest of the mass; but as soon as the air began to act they were destroyed.

The process of refining iron in a reverberatory furnace

shews that carbon is burned off.

Second

Second Experiment; the Fire of the Furnace being assisted by the Vapour of Water. Exp. II.

I had attempted to decompose carburet of iron in small quantities by the help of water in the state of vapour. By heating the carburet red hot, the water was decomposed, and I obtained carbonic acid gas, hidrogen gas, and oxide of iron. As the chief difference between cast and malleable iron consists in a certain quantity of carburet of iron contained in the former, and which must be separated to render the iron malleable, I was desirous of trying the effect of water in vapour on cast iron in a reverberatory furnace, principally in order to know how far the refining of iron might be carried on in this way.

The carburet of iron is decomposed by aqueous vapour.

Application of this principle to refining iron.

About three hundred weight of cast iron of the same quality as before, and just taken from the high furnace, were put into the reverberatory furnace as in the preceding experiment; we then took a large tubulated iron retort, put into it nine or ten quarts of water, fitted a gun barrel to its neck, and introduced the end of the gun-barrel into the little opening in the furnace. The water in the retort was made to boil, so that the steam diffused itself with the flame over the melted metal. At the expiration of half an hour all the marks of refining that had been observed before were perceptible; the ebullition was considerable, and the flame that issued from the chimney more bright. Two hours after the commencement of the process fresh water was put into the retort. In about three hours the metal began to thicken, and at the end of four hours it exhibited the marks of refined iron, and we imagined the operation to be finished. We found the grain of this iron finer, however, than that of the iron operated upon in the preceding experiment, and the mass was full of little blebs.

Steam was introduced,

by which the operation proceeded quickly,

but the iron was of fine grain, full of blebs,

We gave it to the refiner, who treated it like the former; but to our great astonishment we found that it wrought worse in the refinery fire than cast iron the most difficult to refine. It required much more labour, and an hour's time longer.

and was less capable of being refined than before.

Having assayed a specimen in the state in which it came out of the reverberatory furnace, I found it to contain a much larger quantity of oxigen. Experience had already taught me, that half a pound of gray cast iron, treated in a retort with four ounces of charcoal from which all carbonic acid gas had been

There was more oxigen in this than in other kinds of cast iron.

been expelled, gave 32* cubic inches of carbonic acid gas. An equal quantity of white cast iron afforded 165 cubic inches of the same air. Four ounces of the cast iron just taken from the reverberatory furnace, mixed with two ounces of charcoal, yielded 96 inches, or 192 inches to half a pound.

Hence we may infer, that the proportions of oxigen contained in these different kinds of cast iron are,

In iron super-refined by the vapour of water -	192
Common white cast iron - - - - - -	165
Gray cast iron - - - - - - -	96

This super-refined iron imbibed oxigen from the decomposed water which destroyed its carburet.

To the iron produced in the experiment just mentioned, I give the epithet super-refined †, because I conceive it to have been formed as follows:—The water in vapour was decomposed, and destroyed the carburet, as atmospheric air does in the ordinary refining; but at the same time this water imparted to the iron so large a quantity of oxigen, that in the refinery it was necessary, not only to separate the scoria, but to disoxide the metal likewise. This experiment farther confirms the property iron possesses of becoming oxided in different degrees.

If this experiment afforded nothing practically beneficial, it has at least thrown some new light on the properties of cast iron.

Exp. III.

Third Experiment; the Fire of the Furnace being assisted by the Action of Bellows.

Bellows applied with the reverberatory furnace.

The same furnace was used, and the place of the retort in the preceding experiment was supplied by a pair of double bellows mounted with leather, 5 feet (4 f. 8 in.) long, 3 (2 f. 10 in.) broad, and 4 (3. f. 9 in.) high at the posterior extremity when open. It was so placed, that the stream of air was parallel to the flame and to the middle of the furnace, and worked at the rate of eight or ten strokes in a minute. We were desirous of seeing how far the air thus assisted would carry the refining; the furnace being managed and filled as before.

The heat was much greater.

At the end of half an hour the heat was perceived to be much greater than in the first and second experiments. The phenomena of the refining already mentioned appeared in suc-

* Probably this is an error of the press in the original: as it does not agree with the proportion assigned in the next paragraph, one of the two must be wrong.

† Or surcharged with oxigen.

cession;

ceſſion; but inſtead of the frothy ſcoria obtained in the firſt eſſay, a very fluid ſtratum was formed, which diffuſed itſelf over the melted metal, and prevented its refining. This ſcoria, when grown ſolid, was of a blackiſh brown colour and vitreous fracture. We endeavoured more than once to remove it, but the ſtratum was ſo thin as to render it impracticable: As ſoon as one ſtratum was removed, another formed. At the expiration of four hours, the metal being ſtill very fluid, we began to ſtir it, in order to bring its different parts ſucceſſively into contact with the air; this produced an extraordinary heat in the furnace, combuſtion, and ſcintillation, reſembling that which takes place when iron wire is burnt in oxigen gas. This oxidation always produced freſh ſcoria: as ſoon as we deſiſted from ſtirring, every thing became quiet, and the ſtratum of ſcoria prevented the oxidation. At length, after three hours longer, making ſeven in all, during which the melted maſs had frequently been ſtirred, it ſeemed to thicken; perceiving too, that it diminiſhed conſiderably in quantity, the fire was damped; and the matter left to cool in the furnace. It was afterward weighed, and found to have loſt much of its weight. Its extraordinary fracture gave reaſon to preſume a high degree of oxidation; for inſtead of being gray and granulous, it was compact, and of a ſilver white. It was interſperſed with a large quantity of ſpherical cavities, greater or leſs in ſize, which evidently announced the exiſtence of an aeriform fluid, that had been extricated during the fuſion.

The ſcoria melted.

It was blackiſh brown, of a vitreous fracture, and not to be removed.

Stirring produced extreme heat, and ſcintillating combuſtion.

The iron had loſt much in weight.

Its fracture compact and ſilvery.

Porous.

This maſs was too ſmall to be refined. Having examined the quantity of oxigen it contained in the ſame manner as with the other ſpecimens, I found that four ounces yielded 87 cubic inches of oxigen gas, and conſequently nine inches leſs than that which had been treated by means of aqueous vapour. Thus probably here too the oxidation was too powerful, and the iron was ſuper-refined. As the metal did not become doughy in the courſe of the proceſs, it muſt have been ſuperſaturated with oxigen without paſſing through the ſtate of malleable iron. The carburet, it is true, muſt have been totally deſtroyed during the operation, which produced the ſilvery hue.

There was much loſs, and the remainder acquired much oxigen,

without paſſing through the malleable ſtate.

The carburet deſtroyed.

Remarks

VIII.

Remarks on the bursting of two Musquet Barrels by a Charge of Gun-powder confined by Sand. W. N.

A thin musquet barrel burst.

PART of the barrel of a musquet of which the internal diameter was six and a half tenths of an inch, was corked at one end, and fine sand to the depth of twelve inches was poured in: upon this was poured two inches of gunpowder and a small tube (of glass) was then stuck in the gunpowder, and the bore of the tube, which was about one twentieth of an inch diameter, was filled also with gunpowder. The length of the tube was sufficient to reach clearly above the top of the gun-barrel, and all the rest of the space in the barrel, being about thirteen inches above the charge, was filled with sand lightly poured in. In this state the barrel was set up in one corner of a furnace chimney, and a match stuck into the glass tube and lighted, afforded sufficient time for the assistants to remove out of the direct line of explosion before the effect could take place.

The discharge tore the barrel into several contorted pieces in the part near the charge; the upper part fell unaltered, and its contents of sand ran out: the lower part also fell down, but neither its sand nor cork were disturbed, nor was that portion of the barrel affected.

As the thickness of the iron did not exceed one thirtieth of an inch, I was desirous of repeating the experiment with a stronger piece.

A thicker barrel charged with gunpowder and sand.

A musquet barrel, 2½ feet long, diameter of bore five tenths of an inch, and thickness of metal at the breach full one quarter of an inch, was charged with 278 grains, or a little more than half an ounce troy of gunpowder, which occupied the space of four inches. Upon this charge was poured fine sand to the depth of twelve inches, weighing 1151 grains, or about 2¼ ounces troy, and upon this was lightly pressed down a soft wadding of gauze paper, for the purpose of allowing the barrel to be placed horizontally without any subsequent disturbance of its charge. It was safely placed in an horizontal position and fired at the touch hole by means of a train.

Effect of the explosion.

The barrel was torn asunder for the length of eight inches, the part nearest the breach-pin being opened nearly to flatness.

The

The sand remained in the barrel. Its face nearest the blast was consolidated to a very small depth, and I think the mass had been removed or else jammed more closely together; for the space unoccupied between the place where the breach pin had been and the surface of the sand was full nine inches. But as the sand was not immediately noticed, I cannot be sure that none might have been driven or fallen out, during or after the blast; though I am disposed to think not.

It must be remarked, that the powder was a very full charge, and that the sand weighed as much as six musket-balls of half an inch diameter. I do not however apprehend that the barrel would have burst with six balls. Remarks.

The blasting of rocks, the splitting of logs of wood, and the destruction of artillery when on the point of being abandoned to the enemy, are the leading purposes in which the application of sand to confine gunpowder is likely to become useful. Uses.

IX.

Report of a Method of measuring the initial Velocity of Projectiles discharged from Fire-arms, both horizontally and with different Elevations, made to the Physical and Mathematical Class of the National Institute by Mr. PRONY, *Dec.* 11, 1803. *Abridged from the Original* *.

IT is not much above sixty years since experiment began to be applied with success to the theory of projectiles. Mr. Benj. Robins, who may be esteemed the first in this career, employed a pendulum to determine the initial velocity of musket balls, measuring it by the arc of oscillation. He likewise measured the recoil, by suspending the gun-barrel from the pendulum. Robins first examined the velocity of projectiles by the pendulum, and the recoil of a gun by suspending it.

About ten years after, the Chev. d'Arcy published a series of experiments in the memoirs of the French Academy of Sciences, in which he employed two pendulums, against one of which the ball was projected, while the other, to which the gun-barrel was suspended, served to measure the recoil. D'Arcy made similar experiments with two pendulums.

* *Journal des Mines*, No. 92, p. 117, May, 1804.

Fifteen

Dr. Hutton with cannon balls.

Fifteen years after this Dr. Hutton made many experiments at Woolwich with cannon balls by means of the pendulum.

Count Rumford's improvement.

About the year 1778, Count Rumford improved this method of trial, and invented a very simple method of suspending the gun-barrel so that the recoil took place without the axis ceasing to be horizontal.

Dr. Hutton's exp. are the most complete on the subject.

Lastly, Dr. Hutton resumed the subject, and made a number of experiments from 1783 to 1786, with much care, and at great expense, on both kinds of pendulum. These may be considered as forming the most complete and instructive treatise we have on experimental ballistics.

Antoni's description of Mathey's machine.

We have not mentioned the labours of Antoni, but we must not pass over a machine described by him in his essay on gunpowder. This, which he says was invented by a mechanic named Mathey, consists of a horizontal circle, the centre of which is supported by the superior extremity of a vertical axis, and serves as a base to a cylindrical envelope of paper. A rotatory motion is given to this cylinder by means of a cord passing over a leading pulley; and the projectile being discharged horizontally, when the angular velocity of the machine is uniform, in a vertical plane in which the axis is found, pierces the cylinder in two points, the distance of the second of which from the diameter passing through the first measures the arc described by the machine during the passage of the projectile.

Col. Grobert's newly invented machine described.

The machine recently invented by Col. Grobert is constructed as follows:

It consists of two pasteboard disks revolving swiftly at the extremities of an horizontal axis.

A horizontal rotatory axis about 34 dec. (11 feet) long carries at each extremity a pasteboard disk perpendicular to it, and fastened to it so that the whole may turn rapidly without deranging the respective positions of the parts.

A rotatory motion is given to the two disks by means of a weight suspended to the end of a cord, which, after having passed over a pulley ten or twelve yards from the ground, is rolled upon a wheel and axle level with the disks. An endless chain, passing round the wheel and the rotatory axis of the disks, communicates to this axis the motion which the weight in its descent imparts to the wheel.

The advantages this machine possesses over Mathey's consist in the horizontal position of its axis, which admits the utmost degree of firmness and regularity in the position and motion of the

the disks: in the projectile not traversing a cylindrical surface, but two vertical planes, the extent and distance of which may be considerable, and thus give very accurate measures: and its being capable, which no other apparatus is, of measuring the velocities of balls of different sizes projected at different elevations.

The projectile is fired through both disks, and the rotation prevents the second hole from being opposite that on the first pasteboard.

All that is necessary in using this apparatus is to give a uniform and known angular velocity to the disks; and to measure the arc comprised between two planes passing through the axis of the disk, and one of them through the hole in one disk, the other through the hole in its opposite.

Method of using the instrument.

In the trials made, the motion became sensibly uniform, when the weight arrived nearly in the middle of the vertical space it had to traverse, as was found by twice measuring the times of the third and fourth quarters of the descent, and afterwards comparing these times with the corresponding spaces passed through. An excellent stop-watch by Lewis Berthoud, and another by Breguet, were used for this purpose.

The descent of the moving weight becomes uniform.

In most of the experiments the vertical space passed through by the weight was measured by the turns and parts of turns of the cord wound off in a given number of seconds, as in all respects most accurate and commodious.

The space of descent was measured by turns of the cord.

To measure the arc a screen, or pasteboard, was fixed before each disk, a very little distance from it, and the hole in the first disk being brought opposite to the hole in its corresponding screen, a rod carried through the centre of these two holes and of the hole in the other screen which would be opposite them, must pierce the second disk in the plane of the hole in the first; and the arc comprised between this point and the centre of the hole in the farther disk would measure the angle described by the apparatus while the ball was traversing the length of the axis.

Method of measuring the arc between hole and hole.

It is obvious, that the fixed screens, which give the absolute direction of the path of the ball, afford the means of shewing the defect of parallelism, if there be any, between this path and the axis on which the disks revolve.

Two fixed screens are used to shew any defect of parallelism between the path of the ball and the axis.

The gun-barrel was fixed horizontally, parallel to the axis of the disks, and at such a distance, that the concussion of the air by the explosion could not affect the motion of the nearest disk.

The time supposed too short to allow a measurable arc.

One thing may naturally suggest itself, which is, that the time of the ball's passing from one disk to the other, through a space of three or four yards, must be less than $\frac{1}{100}$ of a second; and it is difficult to conceive, that in so short a space the disk could describe an arc capable of being measured.

But it did not prove so.

But this difficulty is easily solved by the fact. When the motion became uniform, the wheel and axle commonly made 0·833 of a turn in a second; and every turn of the wheel produced 7·875 turns of the axis of the disks, which consequently made 6·56 turns in a second. Thus a point on the disk three feet from the axis would move about 41 yards in a second, and in $\frac{1}{100}$ of a second $\frac{41}{100}$ of a yard, or nearly 15 inches, a length more than sufficient for the most accurate measurement.

The fire-arms experimented with.

The experiments were made with a soldier's firelock and a horseman's carbine, the lengths of which in the bore were 3 f. 8 in. and 2 f. 5 in. The balls were accurately weighed, found to be on a medium 382 grains troy, and each was impelled by half its weight of powder.

Formula for calculating the velocity.

The following formula was employed for calculating the velocity of the balls. Putting π for the semiperiphery, when radius is unity $= 3.141$; k for the ratio between the turns made by the wheel and axle and the arbor of the disks; t the time employed by the wheel and axle to make a number of turns n; r the distance of the hole in the second disk from the centre; a the arc described by this hole while the ball passes from one disk to the other; b the distance between the disks; and V the velocity of the ball: we shall have the equation

$$V = \frac{2\pi n}{kt} \cdot \frac{r}{a} b.$$

Mean velocity with a carbine, 1269 f. per sec. with a musket, 1397.

The mean velocity deduced from ten experiments with the carbine was 1269 feet and a half in a second; that from the experiments with the musket, 1397 feet. These being in the ratio of 11 to 10 nearly, it would appear, that the length of the soldiers's firelock might be reduced without much diminishing its range*: but there are other circumstances in a military view, by which the length of the weapon used by the infantry requires to be regulated.

* The differences of the range are much less than those of the velocity. *See Dr. Hutton.*—T.

The

The commissioners made some experiments with half charges or with powder only to the quantity of one fourth of the weight of the ball. In these the mean velocities were, for the firelock 829 feet, for the carbine 822½. These velocities do not differ so much from each other, and considerably exceed the half of those given by the full charge, which may be ascribed chiefly perhaps to the more complete firing of the powder.

With ¼ of powder they were 822½ and 829.

The commissioners were desirous likewise of making some experiments on the resistance of the air to the motion of the ball, the diameter of which was from 15 to 16 millemetres (5·8755 lines to 6·2672.) For this purpose the mouth of the gun-barrel, which at first was 7 f. 9 in. from the nearest fixed screen, was removed to the distance of 67 f. 9 in. In this situation the mean velocity of the musket-ball was 1127 f. instead of 1397, so that it was diminished nearly in the ratio of 42 to 34. The experiments of this kind however were few in number.

The resistance of the air in 20 yards diminished the velocity nearly one-fifth.

There is no doubt but the dimensions of Col. Grobert's apparatus may be enlarged, so as to adapt it to experiments with cannon balls; though it is not easy to say without trial what dimensions would be compatible with accuracy of experiment.

The apparatus might be enlarged with advantage.

The Colonel likewise proposes an alteration in it, for measuring the velocity of projectiles at different elevations, as far as 45°. The following is his contrivance for this purpose. Each of the disks has a separate axis. The wheel and axle has a wheel at each end, with an endless chain, one turning the arbor of one disk, the other that of the other. Thus the rotatory motion imparted by the descending weight is communicated equally to both disks at the same time, the wheels and arbors being made exactly of corresponding dimensions. The stand of the disk farthest from the gun is moveable in a vertical direction, so that it may be raised to the necessary elevation; a few links being added to the endless chain for every different height. As the disk is raised indeed, it becomes inclined to the path of the ball; but as the greatest diminution that can take place in this way is in the ratio of about 7 to 5, a sufficient field is still left for pointing with precision.

Mode of adapting it to different elevations.

To

Additional machinery for counting time, &c.

To prevent any miſtake from want of attention in the perſons employed, Col. Grobert has added certain pieces of mechaniſm to his apparatus, by means of which the weight, when it has deſcended to a certain point, puts in motion a ſecond pendulum to count the time, and a ſyſtem of wheels and pinions connected with the wheel and axle to indicate the number of turns made by it. By ſimilar contrivances it diſcharges the gun, and ſtops the pendulum and the counter of the turns at the proper time. Theſe may occaſionally be of uſe, but complicated machinery is always liable to get out of order, and it may be diſpenſed with here, if the obſervers be ever ſo little expert and attentive.

The motion of the diſks does not affect the path of the ball.

It might be ſuſpected, that the motion of the firſt diſk would cauſe ſome deflection of the ball from its true path before it reached the ſecond. To aſcertain this, three ſcreens were fixed at equal diſtances, the ſecond and third being placed before the firſt and ſecond diſks reſpectively. Now it is obvious, that the hole in the third ſcreen would not be in the ſame vertical plane with thoſe made in the firſt and ſecond, if any deviation took place.

Experimental proof.

A ball being fired through the apparatus thus arranged, a plumb line was ſuſpended before the centre of the hole in the firſt ſcreen, and the moſt accurate obſervation could not diſcover any deviation, but that the ſame line cut the centres of all the three holes. This experiment was ſeveral times repeated with the ſame event.

This owing to the velocity.

The fact no doubt is, that the extreme ſhortneſs of the time, (for the ſemidiameter of the ball is not the forty thouſandth part of a ſecond paſſing the diſk) does not allow the diſk ſenſibly to affect the path of the ball; much leſs can the ball have any effect on the motion of the diſk.

It may not be amiſs to obſerve, that the diſtance of the fartheſt ſcreen being about twelve yards only, the inflexions obſerved by Robins in diſtances of a hundred yards were not likely to take place.

Fact

X.

Fact concerning the invisible Emission of Steam into the Air.

W. N.

SOON after Mr. Giddy had mentioned to me the very remarkable and curious facts of which an account is given at page 1 of the present Number, I was engaged in the experiments on the simmering of water related at p. 216 of Vol. X. I then made an experiment which may perhaps in a small degree elucidate those phenomena. A small glass tube was stuck through a cork, and this was then pressed into the neck of the retort in which water was boiling over the lamp. The steam was emitted through this smaller aperture in a visible jet of upwards of a foot in length. But when a candle was held with its flame immediately beneath the end of the tube, the jet became perfectly invisible. To determine whether the water might be decomposed, or the steam simply expanded so far as to be absorbed by the air, or if condensed to form a vapour too thin to be perceived, I suffered the hot invisible current which had passed through the candle, to pass through a larger glass tube. In this case visible steam issued plentifully from the farther end: Hence, I am disposed to judge that the large tube having kept the very hot steam together and cooled it so as to render it visible again, there was little if any decomposition of the water. But at the same time, when we consider the disappearance of the dense smoke in Mr. Giddy's experiment, there seems to be great reason to think that the charcoal was oxigenated and gazified. If so, the products must have been expanded and invisible steam, hidrogen and carbonic acid. By collecting the products in an experiment of this kind, these conjectures will either be verified or refuted. If the former, we shall have the decomposition of water and oxigenation of carbon at a lower temperature than has hitherto been shewn or expected.

Steam was visibly emitted in a current;

It became invisible when it passed above the flame of a candle;

but the steam was not decomposed;

Perhaps some part may have been changed.

XI.

Experiments made with the Water blowing Machines of the Iron Works of Poullaouen; by Citizens BEAUNIER *and* GALLOIS, *Mine-Engineers* *.

The experiments made to shew the effects of a blowing machine.

OUR object was to ascertain the differences in density of the air within a blowing machine, under the various circumstances by which it might be affected; and at the same time, we endeavoured to find what may be the most advantageous mode of constructing the machine, to produce the greatest effect with the least expenditure of water.

Former accounts are obscure.

One of the chief causes of the doubts that have arisen respecting the suppression or retaining of certain arrangements in the construction, was the omitting to describe the machines, the experiments with which have been compared. We shall therefore previously notice the principal distinctions that may be made between these machines, from the manner in which their effect is produced.

Two kinds of water blowing engines, as the air is received at top, or from the side.

Dr. Lewis observes, that there are two general methods of causing the air to be conveyed by the water in the blowing machines. In the first, the water receives the air by the summit of the machine; in the second it receives it by lateral apertures: and he lays it down as a principle, that those circumstances, which promote the effect in the one case, are detrimental to it in the other.

General observations of Dr. Lewis. The engine is an upright pipe through which a shower of water and air descended.

He observed further, that if the water be at rest in the funnel of the machine, (see Plate III. Fig. 2.) and afterward have liberty to run off, it carries little or no air with it; that if the water have a gyratory motion in the funnel, it carries down a considerable quantity; and that if it fall from a certain height, so as to have been greatly divided, it carries still more: that if the water flow through a pipe with lateral apertures, it receives air through these apertures, even when its motion is slow: that if the pipe be of equal diameter throughout, the quantity of air thus received is inconsiderable;

Lateral apertures preferable for admitting the air.

but if the diameter be diminished to a certain degree at the part where the apertures are made, the quantity of air is greater than could have been introduced through the funnel without

* Translated from the Journal des Mines, No. 91.

without any lateral openings to the air: lastly he observes, that air conveyed downward from the top of the tube, or the funnel, prevents the introduction of the fresh air by the lateral apertures, which in this case, instead of receiving more air, let that which has been already introduced escape.

Lewis concludes, that the two methods by which air may be made to descend with a stream of water, ought not to be united in one machine; and that the machine constructed with a pipe, a funnel, and apertures to let the air enter around or below the throat, produces the most powerful effect.

The machine on which we made our experiments was of the construction which Dr. Lewis has deemed most advantageous. See Fig 2. The machine of Poullaouen described.

The height of the fall, taken from the bottom of the channel that conveys the water to the upper part of the barrel B, is 21 feet 6 inches. Height of the fall.

The height of the funnel, from the bottom of the same channel to the throat *x*, is seven feet. This funnel is of the shape of a frustum of an inverted cone, the larger diameter of which is 12 inches, the smaller four. The remainder of the tube down to the barrel, is a cylinder eight inches in diameter. Funnel at top of the pipe.

The plank N, 12 or 13 inches wide, is fixed one foot below the head of the barrel. The barrel is six feet high. Barrel or air vessel.

The water issues out of the barrel by the triangular apertures *t t t*, and is conveyed away to a drain by the channel M, the bottom of which is four feet higher than that of the barrel. The water flows off beneath;

The air compressed by the external water, the level of which, as will soon be proved, is from 27 to 30 inches above that of the water in the barrel, escapes through the tube P, which is a hollow cylinder five inches in diameter. and the air is conveyed through a pipe at top.

This tube P, called also the air-pipe, terminates in a conical nozzle, having an aperture of two inches only. Air-holes in the upright pipe.

Immediately below the throat *x*, are four air-holes *y y*.

This being premised, we proceed to the instrument employed by us for determining the density of the air in the machines.

It was invented by Citizen Vergnies Bouischére, proprietor of the iron works at Vic-Dessos, in the ci-devant county Gage for measuring the density of the air.

county of Foix, and is a particular kind of barometer, to which the name of water anemometer has been given. See Fig. 6.

It is a short barometer gage inserted, the fluid being water.

It is composed, 1st. of a cylinder A; 2d. of a tube c, bent twice, the lower extremity of which is slightly conical, and terminates about two inches below the bottom of the cylinder; 3d. of a graduated tube d inserted in a vertical position into the cylinder, and reaching below the level n of the water contained in it.

The tube c being inserted into an auger hole made in the side of the blowing machine, and stopping that hole closely, the internal compressed air communicates with the water contained in the anemometer, presses upon it, and in proportion to its density raises to a less or greater height in the graduated tube.

The cylinder A and the tube c are of tin. The lower part of the tube d to the height of nine inches is of the same material, to which is fitted a glass tube about 36 inches long.

The cylinder A is four inches high and as many in diameter. The greatest diameter of the curved tube is half an inch, the smallest, at the extremity, a third of an inch. On observing however, that the size of this opening contributed to increase the extent of the oscillations in the graduated tube, we endeavoured to diminish it as much as possible. For this purpose we closed the lower part of the tube c with sealing wax, in which we afterward made a very small aperture by passing a heated needle through it.

The tube d was divided by a scale of inches, beginning from the surface of the water contained in the cylinder *.

ACCOUNT OF THE EXPERIMENTS.

Experiments with the blowing machine.

I. *Experiments relative to the Expenditure of Water, and the Quantity of Air disengaged.*

The blowing machine No. 1, see the horizontal projection, Fig. 1. to which for the sake of clearness we shall refer our different experiments, served for the trial. It was placed in a T, opposite the machine No. 2, destined for the same purpose.

* The great difference between the diameter of the tube d, and that of the cylinder A allows the level n to be considered as constant.

pose. The afflux of water into each was regulated by means of the hatches *a* and *b*, and the distant floodgate Q, placed in the principal channel D. See the plan Fig. 1. Plate ,

The anemometer was placed in *o*, Fig. 2, in the direction of the vertical tube P protracted. The hatch placed in *b* was let down, so as to prevent the passage of the water that way. The hatch placed in *a* was raised, and the flow of water regulated by means of the floodgate Q. This flow we varied, till we found the water in the graduated tube raised as high as could be effected without any other change of circumstance. When we were certain we had attained this point, and that no variation in the quantity of water flowing off took place, we made the following observations.

1. The mean depth of the water in C, in the little channel, just before the T, was 15 inches 6 lines. Observations.

2. The mean depth of the water in the great channel, was 18 inches 9 lines.

3. The water rose to 26 inches in the graduated tube. The oscillations varied between 25 and 27 inches, but seldom reached the latter height.

4. The velocity of the water in the great channel having been observed, the following data were obtained.

Examined by means of simple floaters of paper, on an extent of 24 feet, we had,

1st. The space passed through in two minutes = 61 feet, 8 inches, 6 lines.

2d. The space passed through in four minutes = 120 feet, 6 inches.

The same velocity examined with cork floats, supporting little balls of wax, the weight of which was augmented by bits of lead, so that they swam in the middle of the stream with a gravity little exceeding that of water, we had for a mean of the space passed in two minutes, 63 feet, 7 inches, 4 lines. Method of measuring the velocity of the water.

If we compare these different results, we shall find, that the mean velocity of the water may be estimated at 30 feet, 11 inches, 1 line, a minute: but as it appears to us, that the results afforded by the cork floats must approach nearest the truth, we will pay no regard to the quantities before obtained, and estimate the mean velocity of the water in the greater channel, at 31 feet, 9 inches, 8 lines, a minute.

Consumption of water.

Now the breadth of the channel employed is 3 feet, 6 inches, and we obferved, that the current, the velocity of which we have given, is 18 inches 9 lines deep. Hence we may conclude, that the confumption of water by the machine, under the circumftances above mentioned, is 173 cubic feet in a minute, the height of the column of water in the inftrument being 26 inches.

Quantity of air emitted.

From the method defcribed in the Hydrodynamics of Boffut, we calculated the quantity of air which this mafs of water caufes to iffue from the machine in a given time. This quantity of air was found to be 7.35 cubic feet in a fecond, or 441 in minute *.

II. *Experiments on the Effect of the Air-Holes.*

Effect of the air-holes afcertained by experiment.

1. On ftopping the four air-holes, the water in the tube of the inftrument defcended to nine inches, and ofcillated very little. The efflux of the water from the machine acquired a velocity fufficient to diminifh the depth of the water in the little channel C, Fig. 1 and 2, near the T, fix inches.

2. One of the air-holes being opened, the water in the tube ofcillated between 22 and 24 inches. The mean = 23 inches.

3. A fecond air-hole being opened, the mean height of the water in the tube was 25 inches.

4. A third air-hole being opened, the columns of water in the tube rofe to its former height of 26 inches.

5. The fourth hole being opened, no perceptible alteration in the inftrument took place, which proves, that this hole has no effect on the machine.

III. *Experiments on the Ufe of Croffes placed at the fuperior Orifice of the Machine.*

ther crofs bars in the top of the tube be advantageous.

Some iron-mafters are accuftomed to place two round bars in the form of a crofs at the upper orifice of the funnel of the machine. Thefe they imagine increafe the effect of the machine by dividing the water at the moment of its fall.

Cylindrical bellows of Namur give more air with lefs water.

* If thefe refults be compared with thofe of the cylindrical bellows of the country of Namur, defcribed by Cit. Bailbet, in No. 16, of the Journal des Mines, it will appear, that, to give out an equal quantity of air, the quantity of water expended by the blowing machines, with a fall more than twice its height, is nearly double that employed to move the cylindrical bellows.

To

To judge of this in the case before us, we fitted in one of these crosses, all the other circumstances remaining as above, and then observed the progress of the instrument.

The column of water in the tube frequently descended to 24 inches, and seldom rose to 26: whence we may estimate the mean height, which before was 26 inches, only 24¾. They diminish the effect.

Now this difference occasions a diminution of velocity in the efflux of the air, and consequently shews the faultiness of this method under the circumstances here mentioned.

IV. *Experiments on the Effect of Hatches placed near the Orifice of the Machine.*

The hatch *a* Fig. 1. was replaced in the grooves adapted to the channel. We altered its height from the bottom of the channel, observing the movements of the anemometer, in order to find the position most favourable for the effect of the machine. Advantage of regulating the influx of water.

The mean height of the column of water in the tube never exceeded 28 inches, the elevation of the lower part of the hatch above the bottom of the channel, being then five inches one line; and it is remarkable, that the difference of a single line in this elevation lowered the water in the tube considerably.

V. *Experiments on the Crosses when the Hatch is used.*

The hatch being placed as has just been said, we fitted the cross again at the superior aperture of the funnel, when the water in the tube of the anemometer sunk. We then varied the height of the hatch above the bottom of the channel; observing the progress of the instrument, to determine the most advantageous position for it under the present circumstances. When thus regulated.

The elevation of five inches eight lines was now found the most favourable to the effect. With this the water oscillated in the tube between 28 and 30 inches, most frequently reaching the latter height, which we could never bring it to exceed, whatever changes we made in the arrangement of the parts that compose the machine. The cross produced more air,

If we compare the situation of the hatch before the addition of the cross, with that which is most suitable in the case before us, we find an increase of seven lines in the height but expended more water.

 from

from the bottom of the channel: now this addition to the height considerably increases the quantity of water expended by the machine.

Conclusions from these Experiments.

General conclusions. The engine by 173 cubic feet falling through 21 feet, drove out 441 cubic feet of air in a minute, under a pressure of 26 inches of water on nearly two inches of mercury; which is not quite one pound per square inch.

(A.) Under the circumstances related in the first set of experiments.

1. The expenditure of water for the blowing machine with which they were made was 173 cubic feet in a minute.

2. The air emitted from the aperture of the nozzle, being two inches in diameter, when the anemometer was at 26 inches, was 441 cubic feet in a minute.

(B.) Of the four air-holes in the machine, three only contribute to the effect produced.

(C.) The hatch placed near the orifice of the machine increased its effect, when the lower part of it was raised five inches one line above the bottom of the channel to which it was fitted.

(D.) A cross placed at the upper orifice of the machine diminishes its effect when the hatch is taken away: on the contrary they increase it, if the hatch be so placed, as to be five inches eight lines above the bottom of the channel, an elevation greater than that mentioned in the preceding paragraph (C.) and which increases the expenditure of water.

From these results it may be inferred, that the cross should not be used in several cases, where the quantity of water with which the machine is supplied, is confined within certain limits.

ANNOTATION. W. N.

The water blowing engine farther explained.

The blowing engine described in the preceding memoir acts upon the principle of the lateral adhesion of fluids, upon which Venturi has so ably written, in a treatise given entire in our Quarto Series of this Journal, and separately published afterwards by Taylor in Holborn. The shower of water in its descent through the vertical pipe carries down a mass of air along with it, in the same manner as a shower of rain on the flat surface of the sea produces that temporary blast of wind,

which ſeamen term a ſquall, and is ſufficiently violent to carry away the maſts of a ſhip, if the ſails be not reduced in time.

It is evident that this engine poſſeſſes the deſireable qualities of cheapneſs and ſimplicity; and Lewis who has written ſomewhat fully upon it, in his Philoſophical Commerce of Arts, from experiments of his own, aſſerts, that it requires much leſs water for working it than any other kind of bellows in uſe. I have no doubt but that many occaſions muſt offer in which it would be beneficial; but whether its expence of water be comparatively ſmall, and its power in any caſe equal to the ſupply of our ſmelting furnaces, may be deſerving of more enquiry.

Numerical ſtatement by Mr. Roebuck, of the effect of a ſteam engine in affording a blaſt of air,

In the excellent paper of Mr. Roebuck on the Devon iron works, inſerted in the fifth Volume of the Edinburgh Tranſactions, and alſo in the Quarto Series of this Journal, there are ſome numerical facts reſpecting the blaſt afforded to iron furnaces by iron cylinder bellows worked by the ſteam engine; and as they agree very well with others given in my Chemical Dictionary, under the article *Trompe*, I will ſtate them in this place. Mr. Roebuck affirms, that one iron furnace was excited by a blowing cylinder, which gave 155 cubic feet of air 16 times per minute, which numbers give a product of 2480 cubic feet. This is 5½ times the quantity emitted by the blowing engine in the text. The ſteam engine was eſtimated to act by a preſſure of 13062 lbs. anſwering to 2¼ lbs. on the ſquare inch of the air piſton, and this multiplied by four feet eight inches, the length of ſtroke, and by 16, the number of ſtrokes, gives 975296 for the weight multiplied by its fall in feet.

compared with the water blowing engine.

Now the machine in the text was worked by 173 cubic feet, or 10812 lbs. of water falling through 21 feet, which gives a product of ~~227052, or more than~~ one fourth of the firſt mover of the ſteam engine blaſt, inſtead of one fifth and a half. The blowing engine therefore conſumed more water by one fourth than would have been required to produce its effect, according to what was done by the ſteam engine But the ſteam engine drove out its air under a reaction of between five and ſix inches of mercury in the gage; a velocity which being more than Mr. Roebuck found neceſſary, was a diſadvantageous waſte of power. The velocity of the water blowing engine produced by its preſſure of two inches, is moſt probably too ſmall; and if ſo, the multiplication of

The ſteam engine has more power.

theſe

these engines would not be adviseable, even if Lewis had been in the right in supposing them to save water. These rough computations, or rather estimates, are sufficiently near for data so loose as those upon which we have operated; and they appear to shew that the principal, and perhaps the only recommendation of the water engines is, that many of them may be made and applied at a small charge, in situations where water with a proper fall is plentifully to be had.

XII.

A Method of rendering the long and short Vibrations of a Balance, governed by a spiral Spring, precisely equal in Duration. By Mr. CHARLES YOUNG. *In a Letter from the Inventor.*

To Mr. NICHOLSON.

SIR,

No explanation has been given why the long and short vibrations of a balance are different.

I HAVE lately tried many experiments upon springs, with a view to obtain some knowledge of the causes which govern an effect that is very troublesome to all makers of chronometers; namely, that the vibrations of the balance through short arcs, consisting of perhaps ninety degrees, are in some instruments performed in longer, and in others in shorter times than those long arcs, such as of four hundred. It is certain that no satisfactory reasons have been given, either in England or in France, to show how this irregularity is produced.

A balance suspended by a strait wire had its long and short vibrations equal.

I made a piece of brass to serve as a large watch balance, and suspended it by a bit of spring wire, on which it could vibrate as an axis, then having turned it four or five times, I left it to regain its natural position *. It performed all its oscillations

* This method of suspension has been used for philosophical purposes, by Mr. Mitchell, (see Priestley's Optics,) by Mr. Cavendish, (see Philos. Trans. and also this Journal quarto II. 416.) by Mr. Coulomb, in his numerous experiments on Electricity and Magnetism; and by Mr. Berthoud, in his Time Piece, No. 24. See his Treatise de la Mesure du Temps, p. 50. It does not appear that this spring has yet been used by itself in time pieces. N.

oscillations precisely in equal times, whatever was their extent, whether they consisted of thirty degrees, or of three thousand. It therefore returned to the place at which it was at rest with velocity exactly proportioned to the forces employed to remove it. From this experiment I concluded, that the balance spring of a watch is not in a situation to exert this natural quality, but that the distortion into which it is thrown, is such as destroy this valuable property of isochronism.

The principal circumstance by which the spiral balance spring appears to me to be cramped, and prevented from operating by its natural action throughout, is, that the outer extremity is fixed by the stud, so that it cannot expand and contract in its coils every where alike, as it ought to do. To remedy this, I attached the stud to a straight spring, lying in the direction of the tangent of the spiral, continued from that extremity. This spring by its easy action allows the spiral to approach the centre, and retire from it with great regularity; and, what is most material, it can with certainty be reduced to such a strength, that the long and short vibrations of the balance will prove perfectly equal when this adjustment is made. For upon the strength of this short spring depends the freedom with which the axis of the balance is enveloped by that spring which regulates its motion. The spring stud affords a good banking; for the banking pin on the balance may be easily placed so as to strike upon the end of the stud in the case of extreme vibration.

The spiral spring is cramped by the stud.

but when its outer end is made free by a straight spring its vibrations are all isochronal.

I am, Sir,

Your's most respectfully,

CHARLES YOUNG.

Wood Street, Aug. 23, 1805.

SCIENTIFIC

SCIENTIFIC NEWS.

Composition of Muriatic Acid.

Letter on the composition of muriatic acid.

IN the third number of the Edinburgh Medical and Surgical Journal, published July 1 last, is the translation of a letter forwarded to the editors of that work by the celebrated Fabbroni. It bears date from Pisa, May 9, 1805, and is written by Dr. Francisco Pacchioni, professor of natural philosopy in the university of that city, to Sig. Lorenzo Pignotti.

Water decomposed by galvanism shewed

After some prefatory observations, the writer announces that he has succeeded by galvanism in obtaining satisfactory evidence of the nature of the constituent principles of muriatic acid. He expresses his confidence that the simplicity of his apparatus and means have secured him against illusion; but for want of time he forbears to relate the whole series of his experiments. His results are,

—that muriatic acid is an oxide of hidrogen.

1. Muriatic acid is an oxide of hidrogen. 2. In the oxigenated muriatic acid and therefore, *à fortiori*, in muriatic acid there is a much less proportion of oxigen than in water. 3. Hidrogen may have very many and different degrees of oxidation.

Some account of the experiment.

The author informs us that having, by accurate experiments, ascertained the true theory of galvanism, he readily discovered a very simple and exact apparatus, in which he could distinctly perceive the changes which happen to water, which, from the continued action of the galvanic pile, is constantly losing its oxigen at the surface of a wire of very pure gold immersed in it.

Water deprived of oxigen first became acid;

With this apparatus, which I conjecture must have been the same as that of Davy, in which the oxigen and hydrogen were given off in separate vessels of water, he observed that pure oxigen was emitted from the gold wire, that the water became acid, and when by proceeding in the operation until the residual fluid occupied about half the capacity of the receiver (that is, I presume, when half the fluid in one of the vessels had disappeared) the remainder was found to be of an orange colour, more deep the less quantity of fluid. It resembled a solution of gold. From the lower orifice of the vessel, which was closed with a piece of taffetas and then with double bladder, a smell was emitted of oxigenated muriatic acid. The gold wire appeared corroded. The bit of taffetas which

—and then appeared to have dissolved part of the gold wire.

which had been in contact with the coloured fluid had undergone an action which rendered it easily to be torn. Round the edges of the vessel on the bladder there was a deep purple ring and within that a circular space rendered colourless or white. A drop of the fluid itself tinged the skin of the hand after some hours, of a beautiful rose colour.

The same liquid, possessing constantly the same qualities, was obtained in various repetitions of the experiment. It was shewn to contain a volatile acid by the white vapours which were formed by ammonia placed near it. It threw down a curdy precipitate from nitrate of silver, which the author concludes to have been the muriate of that metal; and from the whole of the facts he deduces the results first enumerated at the beginnig of this abstract, respecting the composition of muriatic acid from water by depriving it of part of its oxigen. He promises to treat of the other oxides of hydrogen in a memoir shortly to appear.

Qualities of the fluid.

It contained muriatic acid.

The origin and nature of the muriatic acid being thus, as the author observes, determined, there is no longer any mystery in its formation, nor in that of the muriatic salts in the vast extent of the ocean.

Hence the origin of the salt of the sea.

The editors of the respectable Journal, from which I have made this extract take notice of the early discovery of Cruickshank (published by him in our quarto series for 1801) that infusion of litmus was reddened by one end of the pile and infusion of Brazil wood rendered purple by the other, which be ascribed to the formation of nitrous acid and ammonia; and they also quote the discovery of muriatic acid being formed by the galvanic action by Mr. Peele of Cambridge, which was announced in Mr. Tilloch's Philosophical Magazine a few days before Professor Pacchioni's letter was published at Pisa. Mr. Peele's letter bears date April 28, 1805. He took a pint of distilled water and decomposed half of it by means of galvanism; the other half, being then evaporated, left a small quantity of muriate of soda or common salt. Great attention had been paid to the purity of the water; and upon a careful repetition the same result was again had. In a postscript he mentions that a friend of his had tried the experiment and succeeded in the same manner.

Acid and alkali observed by Cruickshank in 1801 to be formed by galvanism,

—and common salt by Peele in 1805,

Or muriate of soda.

Literary

Literary and Philosophical Society of Newcastle upon Tyne.

Twelfth report of the Newcastle Lit. and Phil. Society.

THE Literary and Philosophical Society of Newcastle-upon-Tyne have published their twelfth year's report. The spirited union of literature, science and practical research continues to form the character of their proceedings. Their library encreases no less in value than in magnitude, and they have liberally resolved "that the subscribers to the public library at North Shields (and to other similar institutions which "shall afford an equal accommodation to the members of the "Newcastle Society) shall be admitted to the rooms without "introduction on producing to the librarian a certificate of "their being members of such institutions." I will not suppose that any of my readers will consider this information as merely local. The advantages of provincial societies of estimable and well informed men is of high national importance, and it cannot but be of general interest that such enlightened proceedings as are adopted in one part of the kingdom should be known and imitated in every other quarter, where similar circumstances may render them desirable.

Blasting rocks in Mr. Jessop's method,

I have much pleasure in adding the testimony of Northumberland in favour of the improvement in blasting rocks, which Mr. Jessop communicated last December, through the channel of our Journal.

——tried with success in Northumberland.

At* the meeting in April, 1804, Mr. Fogget of Sheriff-Hill reported, that the new mode of blasting with sand, described in the Philos. Journal had been tried by him, and that, contrary to his expectation, it had answered every purpose of the old mode, with a considerable saving of powder, and of more than one-third of the labour, and with an entire freedom from risk.

At the meeting in May, Mr. Fogget presented a section of two holes drilled and prepared for blasting according to the new method: One perpendicular, in which the charge of powder being introduced and the communication-straw placed, the remainder of the hole is filled up with fine dry sand: the other a horizontal or ascending hole; in which the powder and sand, being made up into a cartridge, is in the act of being thrust up to the farthest extremity of the hole, by a blunt-pointed pricker put in by the side of the communication straw;

* Report, page 5.

At

At the meeting in June an account was communicated by Mr. Thornhill of an accident having happened in Gateshead-Park colliery, by which one man lost his life and another had been severely wounded, in consequence of the powder having taken fire in the common mode of stemming, or ramming down the charge with fragments of stone. A case was also cited by Mr. Horn of a person who had lately been brought from Alston Moor with his skull fractured by a similar explosion.

Danger of the old method of ramming down the charge.

New Process for Steeping Hemp.

THE new process of M. Bralle for steeping hemp, which has the advantage of saving time, capital, and the health of numerous individuals, and of which an account was given at page 86 of our last volume, has been repeated in one of the provinces of France, to the entire satisfaction of the inhabitants, who might be supposed the least inclined to deviate from their accustomed habits. The staple was found to be excellent, and of a superior strength and quality when spun into thread, and also after it had passed the loom in the form of cloth.

Steeping of hemp.

Medical Theatre, St. Bartholomew's Hospital.

THE following courses of Lectures will be delivered at this theatre during the ensuing winter.

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and physiology, by Mr. Abernethy.

On the theory and practice of surgery, by Mr. Abernethy.

On comparative anatomy and physiology, by Mr. Macartney.

On chemistry, by Dr. Edwards.

On the materia medica, by Dr. Powell.

Anatomical demonstrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Tuesday, October 1st, at two o'clock, and the other lectures on the succeeding days of the same week.

Further particulars may be learned by applying to Mr. NICHOLSON, at the apothecary's shop, St. Bartholomew's hospital.

AN

Medical Institution.

AN institution has been lately established in London for the purpose of promoting a liberal and useful intercourse among the different branches of the medical profession, and of affording a centre for the reception of communications, and for the formation of a select and extensive professional library. It is called the *Medical and Chirurgical Society of London*, and it comprizes a considerable number of professional men of the first character. The meetings (which will commence in October) will be held at the Society's apartments, Verulam-buildings, Gray's-Inn, where any communications, or donations of books are requested to be sent, directed to the secretaries.

The following is a list of the officers and council for the present year.

PRESIDENT, WM. SAUNDERS, M.D. F.R.S.

John Abernethy, Esq. F.R.S. *Vice-Pres.*
Charles Rochemont Aikin, Esq. Sec.
Wm. Babington, M.D. F.R.S. *Vice-Pres.*
Matthew Baillie, M.D. F.R.S.
Thos. Bateman, M.D. F.L.S.
Gilbert Blane, M.D. F.R.S.
Sir Wm. Blizard, F.R S. *Vice-Pres.*
John Cooke, M.D. F.A.S. *Vice-Pres.*
Astley Cooper, Esq. F.R.S. *Treas.*
James Curry, M.D. F.A.S.
Sir Walter Farquhar, Bart. M.D.
Thompson Forster, Esq.
Algernon Frampton, M.D.
John Heaviside, Esq. F.R.S.
Alex. Marcet, M.D. *For. Sec.*
David Pitcairne, M.D. F.R.S.
Hen. Revell Reynolds, M.D. F.R.S.
H. Leigh Thomas, Esq.
James Wilson, Esq. F.R.S.
John Yelloly, M.D. *Sec.*

Properties

Properties of blued Steel not generally known.

IN making springs of steel the metal is drawn or hammered out and fashioned to the desired figure. It is then hardened by ignition to a low red heat and plunging it in water, which renders it quite brittle. And lastly, it is tempered either by blazing or blueing. The operation of blazing consists in smearing the article with oil or fat, and then heating it till thick vapours are emitted and burn off with a blaze. I suppose this temperature to be nearly the same as that of boiling mercury, which is generally reckoned to be at the 600th of Fahrenheit, though, for reasons I shall in future mention, I think this point requires to be examined. The operation of blueing consists in first brightening the surface of the steel, and then exposing it to the regulated heat of a plate of metal or a charcoal fire, or the flame of a lamp until the surface acquires a blue colour by oxidation. The remarkable facts which I have here to present to the notice of philosophers are that Mr. Stodart assures me that he has found the spring or elasticity of the steel to be greatly impaired by taking off the blue with sand paper or otherwise; and, what is still more striking, that it may be restored again by the blueing process without any previous hardening or other additional treatment.

Method of making springs.

Hardening, blazing and blueing.

A blue spring injured by brightening and restored by blueing again.

Mr. Hardy, who is meritoriously known as a skilful artist,* assured me some time ago that the saw-makers first harden their plates in the usual manner, in which state they are more or less contorted or warped, and are brittle;—that they then blaze them; which process deprives them of all springiness, so that they may be bended and hammered quite flat, which is a delicate part of the art of saw making;—and that they blue them on an hot iron which renders them stiff and springy without altering the flatness of their surface. Mr. H. finds that soft unhardened steel may be rendered more elastic by blueing, and that hard steel is more expansible by heat than soft.

Saw makers harden their steel; then soften it by blazing; and render it elastic by blueing.

Soft steel blued. Hard steel expands more by heat.

It is very difficult to reason or even to conjecture upon these facts. They certainly deserve to be verified by a direct process of examination, which I intend to make, and shall state the results in our next number. N.

* See his banking for time pieces in our XI. Vol. page 114.

Cor-

Preservation of Succulent Plants.

Whether the fact be generally in possession of the collectors of plants I know not, but it will certainly be instructive to many readers to be informed that green succulent plants are much better preserved after a momentary immersion in boiling water than otherwise. This treatment, which I am told is adopted for the economical preservation of cabbage and other plants which are dried for keeping, destroys the vegetable life at once, and seems to prevent an after process of decay or mortification, by which the plant would have been more considerably changed, if it had not been so suddenly killed.

Corrections to the 11th Volume.

P. 159, l. 2, *area* r. *era*.—l. 21, *Agy* r. *Aqy*.—l. 36, $q \times p - 2n$ r. $q + p - 2n$.—l. 12, *for* r. *of*.—l. 28, put a comma before *that*.—p. 236, l. 20 from the bottom, after the words *preserved as* add *as perfect as possible, but the press recommended by Dr. Withering does not appear calculated, &c.*—

Dr. Bostock's essay upon animal fluids which was communicated by the author and inserted in our last number, appeared also in the third number of the Edinburgh Medical and Surgical Journal. This by a casual omission of the friend who forwarded me manuscript was not intimated to me until several days after the paper came to hand.

Engine for raising Water, by Mr. Wm. Close.

Fig. 1.

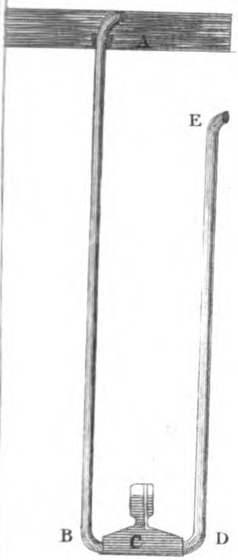


Fig. 2.

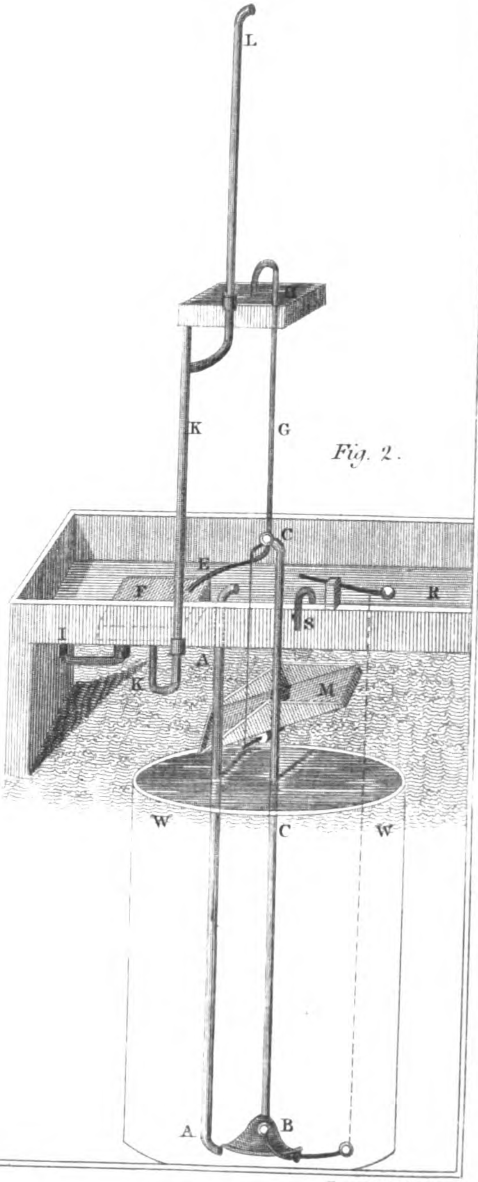


Fig. 3.

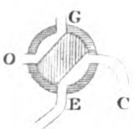


Fig. 4.

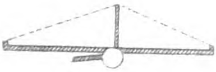
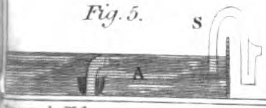


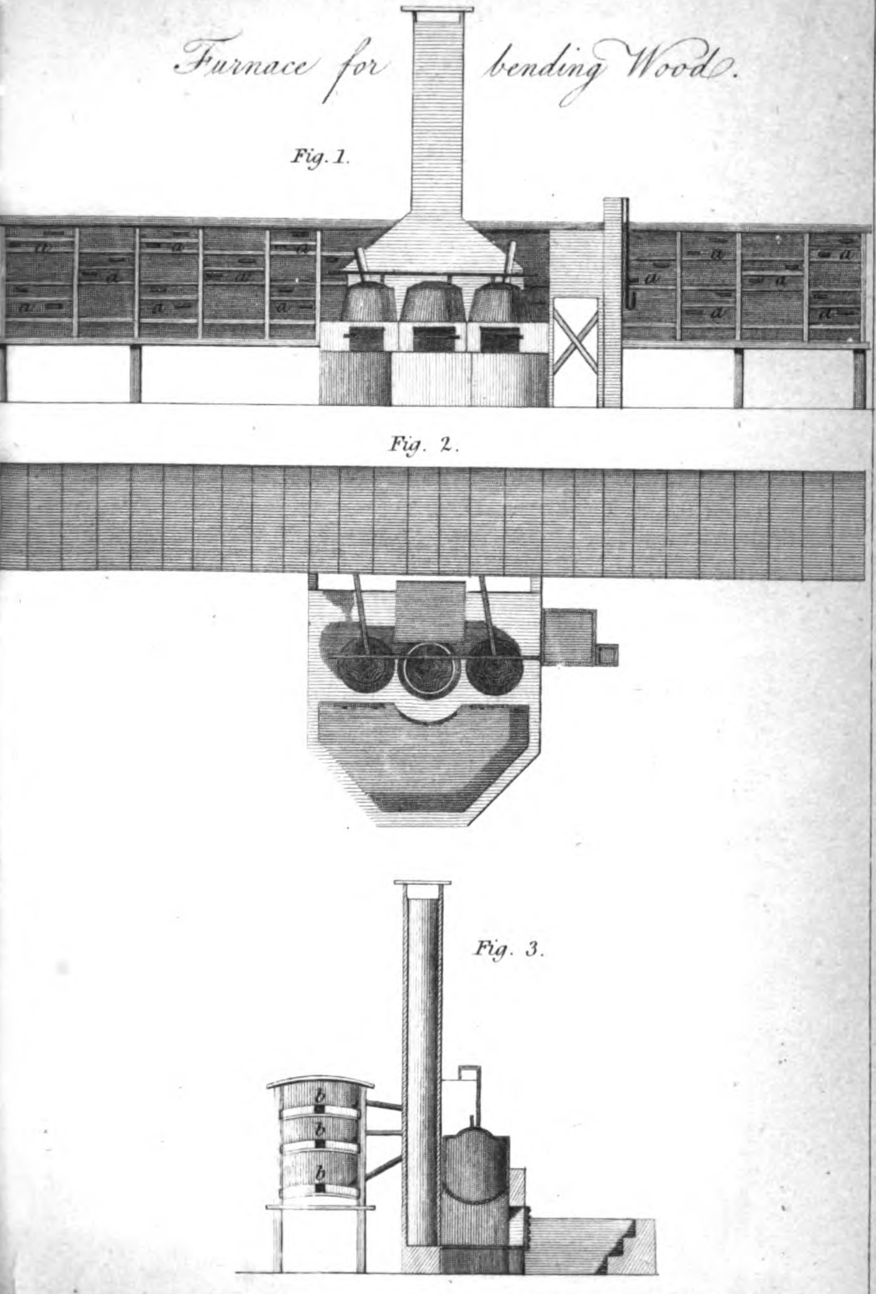
Fig. 5.



Drawn by W.C.

Engraved by Mutlow.

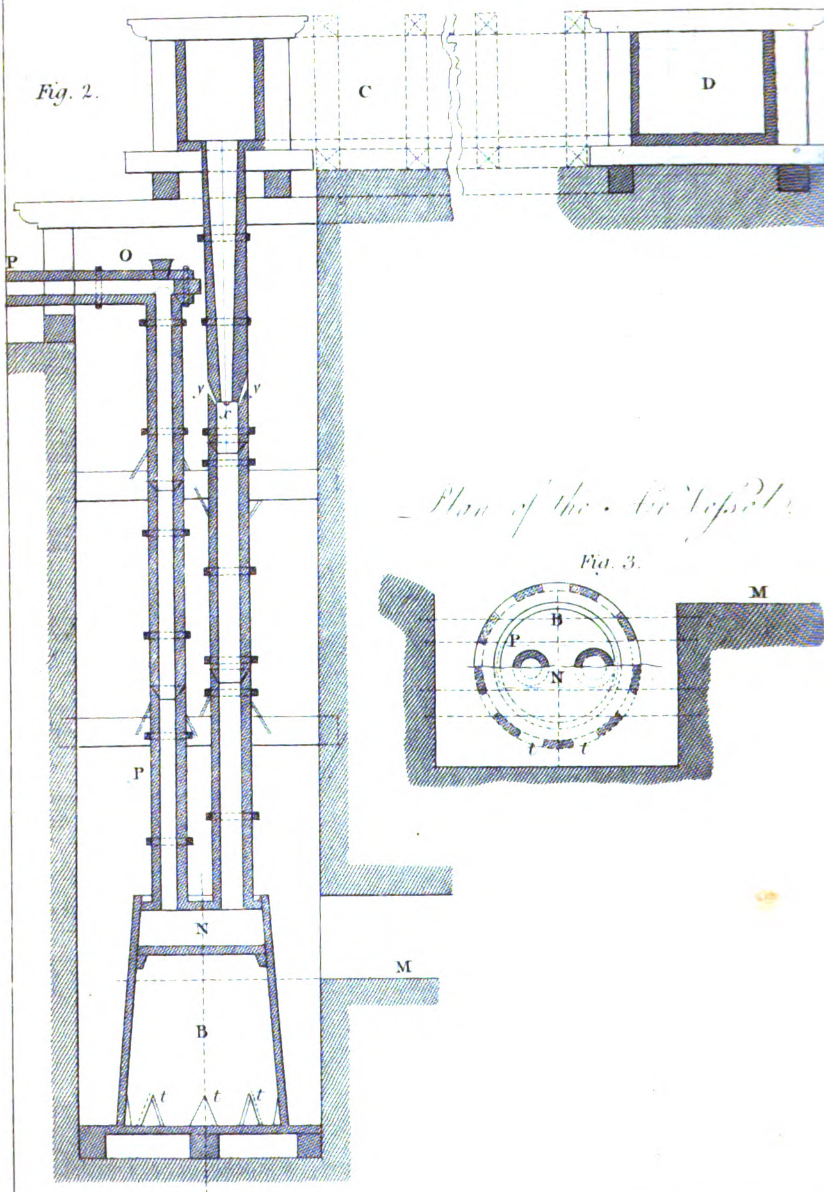
Furnace for bending Wood.

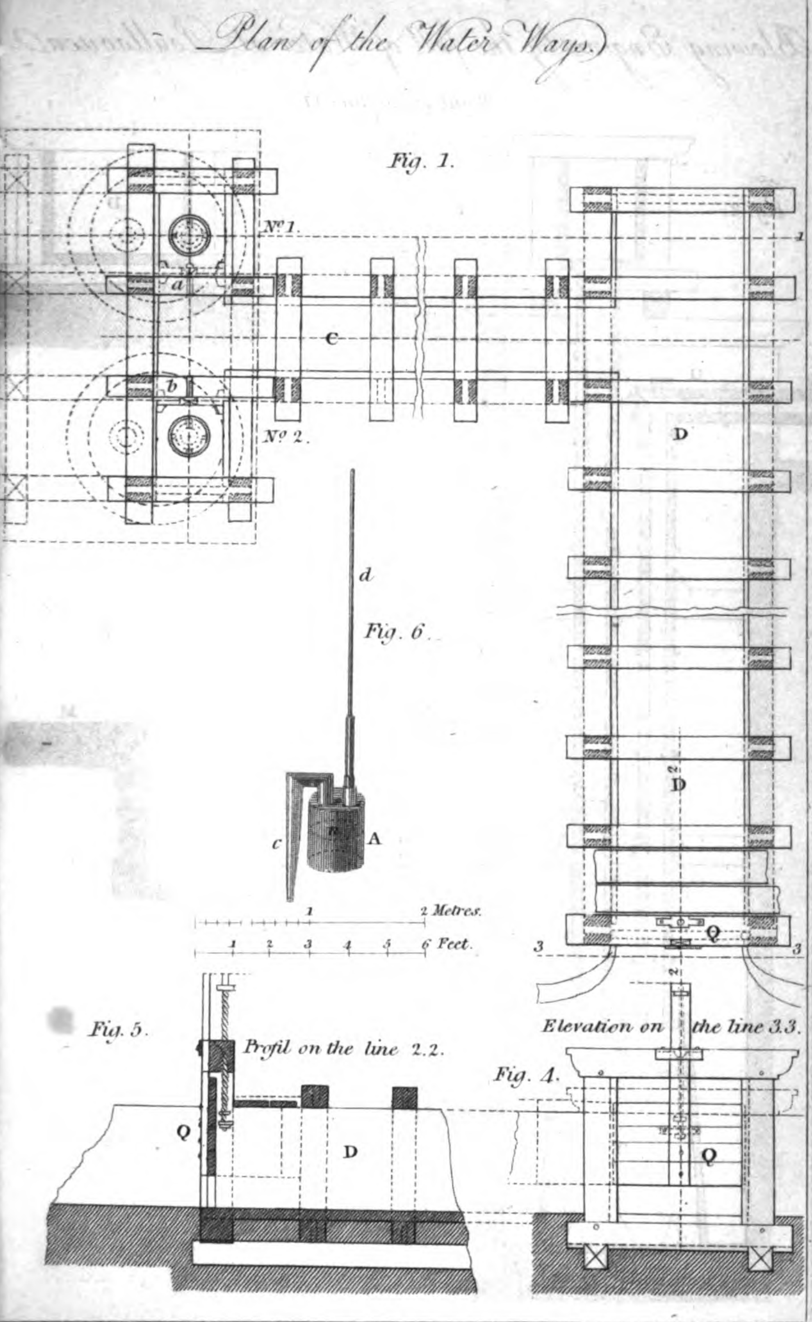


Mutlow Sc. Russell Ct.

Blowing Engine by the fall of Water at Poullaouen.

Profil on the line 11.





Mutlow Sc. Russell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1805.

ARTICLE I.

Experimental Investigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c. Received from the Author.*

To Mr. NICHOLSON.

DEAR SIR, *Munich, August* 29, 1805.

HAVING learned by a letter which I received this day from England, that you have published in your Journal of Natural Philosophy the memoir I sent you on the temperature at which the density of water is a maximum, I take the liberty to send you herewith inclosed three memoirs on heat, which are destined to appear in the next volume of the publications of the first class of the National Institute.—Three other memoirs of mine will appear in that volume, but as they contain little that would be new to you, I do not send them to you.

I continue my researches on heat, and have lately made several new and very interesting experiments, the results of which it is my intention to communicate to you, as soon as I shall have compleated the particular course of experiments in which I am now engaged.

I am, Dear Sir, with much esteem,

Your most obedient servant,

RUMFORD.

Sect. I. *Short Account of a new Experiment on Heat.*

I have lately made a new experiment, the refult of which appears to me fufficiently interefting to deferve the attention of the clafs.

Qu. Whether the heating and cooling of polifhed and of blackened bodies follow the fame law in fmall clofed fpaces as in fpaces more enlarged?

Having found by experiments often repeated that metallic bodies expofed in the free air of a large apartment are much more fpeedily heated and cooled when their furfaces have been blackened (over the flame of a candle for example) than when they are clean and polifhed; I was curious to know whether the fame phenomena would take place when, inftead of expofing thefe bodies in the open air, they fhould be placed in clofe metallic veffels furrounded by a certain thicknefs of included air, and thefe veffels fhould be then plunged in a large mafs of hot or cold water. In order to clear up this important point, I made the following experiment:

A cylindrical veffel of thin brafs was fupported in the middle of a larger veffel, fo as to leave a thin interval of air between them.

A cylindrical veffel of brafs, three inches in diameter and four inches long was enclofed in another larger cylindrical veffel, in the centre of which it was fufpended by its neck, fo as to touch it in no other part, leaving on all fides an interval of one inch between the veffels.

The external veffel as well as the fmaller one included within it is made of thin fheets of brafs; its diameter is five inches and its height fix. It is one inch and a half in diameter and fix inches high. Its neck is one inch and a quarter in diameter and two inches and a half long.

The interior veffel is fufpended in the centre of the external one by a ftopper of cork. This ftopper is adjufted to the neck of the external veffel, and there is a cylindrical hole of three quarters of an inch diameter through the cork, and having the fame axis, which perforation receives the neck of the interior veffel and retains it in its place.

The interior veffel was introduced and fixed in its place before the bottom of the exterior veffel was foldered in.

The larger veffel was fupported on a foot.

At the centre of the bottom of the great veffel is a fmall metallic tube of three quarters of an inch diameter and one inch and a half long, by means of which this inftrument is attached to a folid heavy foot of metal which fupports it in a vertical pofition when the whole inftrument is fubmerged in a veffel of water.

This inftrument, which greatly refembles that defcribed in my 7th effay on the propagation of heat in fluids, which I have called

called the Paſſage Thermometer*, may be uſed to make a number of intereſting experiments on the cooling of bodies through different fluids. In the preſent experiment I employed it in the following manner:

The inner veſſel was filled with hot water, and a thermometer placed in its neck.

The interior veſſel was entirely filled with hot water to the height of half an inch in its neck, and a good thermometer, having its cylindric bulb four inches long, was inſerted therein. The inſtrument was then plunged in a mixture of pounded ice and water, and the time was noted by means of the thermometer, during which the hot water in the ſmall veſſel became cold.

The whole inſtrument was then plunged in ice and water.

I was careful to plunge the inſtrument in this frigorific mixture, ſo that the large veſſel was completely ſubmerged except the upper extremity of its neck; and I added from time to time a ſufficient quantity of pounded ice, to keep the frigorific mixture conſtantly and throughout at the temperature of melting ice.

The following were the reſults afforded by two ſimilar inſtruments employed at the ſame time:

Reſults with two inſtruments one of which, B, had the interior ſurface of the larger veſſel and the exterior of the ſmaller blacked; and the other inſtrument, A, had the like ſurfaces poliſhed.

Theſe two inſtruments, which I ſhall diſtinguiſh reſpectively by the letters A and B, are of the ſame form and dimenſions; there is no difference between them but in the ſtate of their ſurfaces. In the inſtrument A, the exterior ſurface of the ſmall veſſel and the interior ſurface of the great veſſel which incloſes it, are bright and poliſhed; but in the inſtrument B, the exterior ſurface of the ſmall veſſel and the interior ſurface of the large veſſel are black, having been blackened over the flame of a candle before the bottom of the great veſſel was ſoldered in its place.

The interior veſſels contained boiling water.

Having filled the interior veſſel of each of theſe inſtruments with boiling water till the water roſe ſo the height of half an inch in the neck, I placed a thermometer in each, and then plunging both inſtruments at the ſame time into a tub filled with cold water mixed with pounded ice, I obſerved the courſe of their refrigeration during ſeveral hours.

The refrigerating veſſel contained ice and water.

Each of the inſtruments was completely ſubmerged in the frigorific mixture, excepting about one inch of the ſuperior extremity of the neck of the exterior veſſel, and I was careful

* See our Journal, Vol. IX.

to

to add new quantities of pounded ice from time to time, in order to keep the frigorific mixture constantly at the precise temperature of melting ice.

Caution to insure equality of temperature.

As the specific gravity of water at the temperature of three or four degrees of the thermometer of Reaumur, is greater than that of melting ice, the water which lies at the bottom of a vessel containing a mixture of water and pounded ice, is usually warmer than the fluid which occupies the upper part of the vessel. To remedy this inconvenience my refrigeratory for the frigorific mixture was a tin vessel supported on three feet of one inch in length, and I placed this first vessel in a larger of wood, containing a certain quantity of ice surrounding the bottom and part of the sides of the metallic vessel.

Method of observation.

As in the first moments of the experiment the thermometers descended too quickly to be observed with precision, I waited till each of them had arrived at the 55th degree of Reaumur; after which I carefully observed the number of minutes and seconds employed in passing through each interval of five degrees of the lower part of the scale of the thermometer to the fifth degree above zero.

Table of the course of cooling.

The following table exhibits the depression of the thermometers during eight hours employed in the experiment.

Degrees of the Thermometer.			Time employed in cooling. By the Instrument A.			By the Instrument B.	
From 55 to 50		-	11m	6s	-	7m	50s
50	45	-	13	15	-	8	10
45	40	-	15	12	-	9	5
40	35	-	19	10	-	10	50
35	30	-	22	24	-	12	18
30	25	-	27	50	-	15	10
25	20	-	37	6	-	21	15
20	15	-	54	15	-	28	15
15	10	-	80	25	-	41	25
10	5	-	183	45	-	85	15
Time employed in cooling from 55° to 5°,			478	4	-	254	5

It

It is evident from the results of this experiment, that the blackened body is constantly cooled in less time than the polished body; but it appears by the course of the thermometers, that the difference between the quickness of cooling of these two bodies varies, and that this difference was less considerable in proportion as the temperature of the bodies was more elevated in comparison to that of the medium in which they were exposed to cool.

The blackened body always cooled more quickly than the other.

In cooling from the 55th degree to the 50th above the temperature of the surrounding medium, the polished body employed 11^m 6^s, and the blackened body employed 7^m 50^s to pass through the same interval. But from the 10th to the 15th degree above the temperature of the medium, the polished body employed 183^m 45^s, while the blackened body employed only 85^m 15^s; but it is extremely probable that this difference between the proportion of the times employed in cooling the two bodies at different temperatures, is only apparent, and that it depends on the greater or less time required for the thermometers in the vessels to arrive at the mean temperatures of the masses of water which surround them.

The difference was greatest at the lowest temperatures; probably because the thermometers then shewed the mean temperature more correctly.

In order to compare the results of this experiment with those I made last year with metallic vessels polished and blackened, and left to cool in the undisturbed air of a large chamber, it is necessary to ascertain how much time the two bodies in question employed in cooling, from the 50th to the 40th degree of Fahrenheit above the temperature of the medium. Now I found by observation, that the polished vessel A employed 39^m 30^s to pass over that interval of cooling, while the blackened vessel B employed only 22^m. These times are in the proportion of 10000 to 5810. By one of my experiments made last year, I found that the times employed in passing through the same interval of cooling in the open air by a clean polished metallic vessel, and another of the same form and capacity, but blackened without, were as 10000 to 5654.

From these experiments it appears that the rate of cooling in the polished body, compared with the other, is nearly the same as was formerly determined with bodies in a large chamber.

Reflecting on the consequences which ought to result from the radiations of bodies, on the supposition that the temperatures of bodies are always changing by means of these radiations, I was led to the following conclusion: If the intensity of the action of the rays which proceed from a body, be universally as the squares of the distances of bodies inversely, which

If the intensities of radiated heat be inversely as the squares of the distances, bodies will cool in the same time in an enclosure of the same tem-

perature, whether large or small.

which is extremely probable, a hot body exposed to cool in a close place, or surrounded on all sides by walls, ought to cool with the same celerity, or in the same time, whatever may be the magnitude of this enclosure, provided the temperature of the sides or walls be at a constant given temperature; and the results of the experiment here described, in which the hot body was enclosed in a vessel of a few inches diameter, compared with those of several experiments made last year, in which the heated bodies exposed to cool between the walls of a large chamber, appear to confirm this conclusion.

These facts confirm that truth;

and that the air has little effect.

As to the effect produced by the air in cooling a heated body exposed to cool in a close place filled with that fluid, I have reason to believe that it is much less considerable than has been supposed.

Former experiments proved that it receives only 1-27th part.

I have shewn by direct and conclusive experiments, that bodies cool and are heated, and that with considerable celerity, when placed in a space void of air *; and, by experiments made last year with the intention of clearing up this point, I found reasons to conclude, that when a hot body cools in tranquil air not agitated by winds, one twenty-seventh only of the heat lost by this body (or to speak more corectly, which it excites in surrounding bodies) is communicated to the air, all the rest being carried to a distance through the air, and communicated by radiation to the surrounding solid bodies.

The rest passes by radiation.

Sect. II. *Experiments on cooling Bodies.*

It is only by careful observation of the phenomena which accompany the heating and cooling of bodies, that we can hope to acquire exact notions of the nature of heat and its manner of acting.

Conducting power of bodies with regard to heat.

Many experiments have been made by different persons at different times, with a view to determine what has been called the conducting quality of different substances with regard to heat: I have myself made a considerable number; and it is from their results, often no less unexpected than interesting, that I have been gradually led to adopt the opinions on the nature of heat which I have presumed to submit to the judgment of this illustrious assembly. The flattering attention

* See my Memoir on Heat in the Philosophical Transactions for 1786, and in my eighth Essay.

with

with which the Clafs has honoured the three Memoirs I have lately prefented, encourages me to communicate the continuation of my refearches.

All philofophers are agreed in confidering glafs as one of the worft conductors of heat which exifts; and when it is propofed to confine the heat in a body of which the temperature has been raifed, or to hinder its diffipation as much as poffible, care is taken to furround the heated body with fubftances known to be bad conductors of heat.

Glafs is allowed to be one of the worft conductors.

The refults of many of my experiments having led me to fufpect that the cooling of bodies is not effected in the manner which is generally fuppofed, I made the following experiment with the intention of clearing up this interefting part of the fcience.

Bodies are not, probably, cooled by conducting off.

I procured two bottles nearly cylindrical, of the fame form and the fame dimenfions when meafured externally; one being of glafs and very thick, and the other of tin or tinned iron, which was very thin. Each of them is three inches ten lines in diameter very nearly, and five inches in height, and each has a neck one inch three lines in diameter, and one inch two lines in height. The glafs bottle weighs 13 ounces 1 gros and 18 grains poids de marc, and the other thin metallic veffel weighs only 5 ounces 1 gros and 65 grains.

Experiment: A thick glafs bottle and a thin one of tin,

Having very exactly weighed the bottle of tinned iron, I found its exterior furface to be 54,462 inches, which give 0,21142 of a line for the thicknefs of its fides, taking the fpecific gravity of the metal at 7,8404.

were prepared, weighed,

The mean thicknefs of the fides of the glafs bottle is more than fix times as great, as may be eafily deduced from a calculation founded on the weight of the bottle, the quantity of its furface, and the fpecific gravity of glafs.

and meafured.

Having filled thefe two bottles with boiling water, I hung them up by flender ftrings in the midft of the tranquil air of a large chamber, at the height of five feet from the floor, and at the diftance of four feet afunder.

They were filled with hot water, and left to cool in the air.

The temperature of the air of the chamber, which did not vary a quarter of a degree during the whole time of the experiment, was $9\frac{1}{4}$ degrees of Reaumur's fcale.

An excellent mercurial thermometer, with a cylindrical bulb, of four inches long and two lines and a half in diameter, fufpended in the axis of each of thefe bottles, indicated the temperature

temperature of the contained water; and the time employed in its cooling for every five degrees of Fahrenheit's thermometer, was carefully obſerved during eight hours.

The glaſs being conſidered as a very bad conductor of heat, and the ſides of the bottle being ſo thick, who would not have expected that the water in this bottle would have been more ſlowly cooled than that in the very thin bottle of tin.

The glaſs bottle cooled twice as quick as that of tin.

The contrary however was the event; the bottle of glaſs was cooled almoſt twice as quickly as that of tin.

While the water included in the bottle of tinned iron employed 56 minutes to paſs through a certain interval of cooling, namely through ten degrees, between the 50th and 40th degree of the thermometer of Fahrenheit above the temperature of the air of the chamber, the water in the glaſs bottle employed only 30 miuntes for the ſame change.

Inference.

It appears to me, that the reſult of this experiment throws great light on the myſterious operation of the communication of heat.

Heat is not likely to be a material ſubſtance.

If we admit the hypotheſis that hot bodies are cooled, not by loſing or acquiring ſome material ſubſtance, but by the action of colder ſurrounding bodies, communicated by undulations or radiations excited in an etherial fluid, the reſults of this experiment may be eaſily explained; but if this hypotheſis be not adopted, I cannot explain them.

Bodies are not cooled by the ſurrounding air.

It might perhaps be ſuſpected that the air attached by a certain attraction, but with unequal forces, to the ſurfaces of the two bottles, might have been the cauſe of this remarkable difference in the time of their cooling; but thoſe who will take the trouble to reflect attentively on the reſults of the experiments I have deſcribed in a preceding memoir, which were made with a view to clear up this point, with a metallic veſſel firſt naked, and afterwards with one, two, four and five coatings of varniſh, will be perſuaded that this cauſe is not ſufficient to explain the facts.

All metallic veſſels have the ſame diſpoſition to cool.

By a courſe of experiments made at Munich laſt year, of which the details are given in a Memoir ſent to the Royal Society of London *, I have found that a given quantity of hot water included in a metallic veſſel of a given form and capacity, always cools with the ſame quickneſs in the air,

* See our Journal, Vol. IX. p. 194.

whatever

whatever may be the metal employed to conſtruct the veſſel; provided always that the external ſurface of the veſſel be very clean, and the temperature of the air the ſame.

In order that the cooling ſhall be effected in the ſame time, nothing more is required than that the external ſurface of the veſſel be truly metallic, and not covered with oxide or other foreign bodies.

The ſurface only need be metallic.

On the enquiry, what quality all the metals might have in common, and poſſeſs in the ſame degree, to which this remarkable equality of their ſuſceptibility of cooling might be attributed, I found it in their opacity.

This ariſes from the opacity of metals,

The rays which cannot penetrate the ſurface of a body, muſt neceſſarily be thrown back or reflected; and as the rays of light, which have much analogy with the inviſible calorific or frigorific rays, eaſily penetrate glaſs, though they are reflected, at leaſt for the greateſt part, by metallic ſurfaces, I ſuſpected beforehand the reſult of the experiment with the two bottles, one of glaſs and the other of tinned iron.

by which the heat is reflected.

The ſtate of a heated body, or a body which contains a certain quantity of caloric, has been compared to that of a ſponge which contains a certain quantity of water. Suppoſing this compariſon to be juſt, we might compare the loſs of heat by the emiſſion of the calorific rays, to the loſs of water by evaporation. Let us try if this compariſon can ſupply us with the means of throwing ſome light on the intereſting ſubject of our reſearches.

Uſual compariſon of heat to water in a ſponge.

Inſtead of the ſponge filled with water, let us ſubſtitute the earth, and ſuppoſe for a moment that the earth is every where eqaally heated, and its ſurface in all parts covered with a bed of the ſame kind of ſoil equally moiſt.

The ſame amplified.

As a ſquare league in a mountainous country contains more ſurface or more ſuperficial acres than a ſquare league ſituated in the plain, it is evident that more water would be evaporated from the whole ſurface of the earth in a given time if the earth were covered with mountains, than if its ſurface were an immenſe plain, and conſequently, that more caloric ought to be projected from the ſurface of any ſolid body broken with aſperités, than from the ſurface of another body of the ſame form and dimenſions, which is ſmooth or well poliſhed.

If it were true, a rough ſurface would emit more heat than a ſmooth one.

This

But the facts are contrary.

This reafoning appears to me to be juft, and if I am not deceived, the conclufions which may be drawn from the facts in queftion, well confirmed by experiment, ought to be confidered as demonftrative. I have taken every poffible care to eftablifh thefe facts; and the refults of all my experiments have conftantly fhewn that more or lefs perfect polifh, or the greater or lefs brightnefs of the furface of a metallic veffel, does not fenfibly influence the time of its cooling.

A polifhed and unpolifhed veffel of brafs cooled in the fame time.

I took two equal veffels of brafs and polifhed the external furface of one of them as highly as poffible; and I deftroyed the polifh of the other by rubbing it in all directions with coarfe emery. When thefe two veffels were filled with hot water, I did not find that the unpolifhed veffel employed more or lefs time in cooling than that which was polifhed.

Caution.

I was careful to wafh the furface of the unpolifhed veffel effectually with water before the experiment; as I knew that if I did not take the precaution of removing all the dirt which might be lodged in the afperités of the furface, the prefence of thefe fmall foreign bodies would influence the refult of the experiment in a fenfible manner.

A rough furface may reflect as much light as a fmoother.

We ought carefully to diftinguifh thofe furfaces which appear unpolifhed to our eyes, but which in fact are not fo, from thofe which reflect little or no light.

Metals not lefs reflective for lofing their polifh.

It is more than probable that the furface of a metal is always polifhed, and even always equally fo in all the cafes wherein the metal is naked and clear and clean, notwithftanding all the mechanical means which may be ufed to fcratch its furface and break the glare of its luftre.

If the radiation of heat defcend on furrounding bodies, it will be of no confequence whether the radiating body be polifhed or not.

Let us return to the comparifon of the evaporation of water from the furface of the earth, with the emiffion of caloric radiating from the furface of a heated body, and let us fuppofe for an inftant, that the evaporation of the water from the furface of the earth does not depend on the heat of the earth itfelf, but that it is caufed merely by the influences of furrounding bodies, as for example, by the rays of light received from the fun. It is evident that, in this cafe, the evaporation could not be fenfibly greater in a mountainous country than in the plain; and by an eafy analogy we fee, that if hot bodies be cooled, not in confequence of the emiffion of fome material fubftance from their furfaces, but by the pofitive action of rays fent

fent to them by colder furrounding bodies the more or lefs perfect polifh of their furfaces, ought not fenfibly to influence the rapidity of their cooling.

This is precifely what all my experiments concur to prove. I have long fought, and with that patience which the love of the fciences infpires, to reconcile the refults of my experiments with the opinions generally received concerning the nature of heat and its mode of action, but without being able to fucceed.

Experiments prove this pofition.

It is in the hands of two of the moft illuftrious bodies of learned men that ever exifted that I have thought it incumbent on me to depofit my labours, my difcoveries, my doubts, and my conjectures.

I am earneftly defirous of engaging the philofophers of all countries to turn their attention towards an object of enquiry too long neglected.

The fcience of heat is not only of great curiofity, from the multitude of aftonifhing phenomena it offers to our contemplation, but it is likewife extremely interefting from its intimate connection with all the ufeful arts, and generally with all the mechanical occupations of human life.

Importance of the fcience of heat.

Without a knowledge of heat it is not poffible either to excite it with economy, or to direct its different operations with facility and precifion.

(The Remainder in our next.)

II.

On pure Nickel, difcovered to be one of the noble Metals, and on its Preparation and Properties. By J. B. RICHTER.*

ON repeatedly cryftallizing fulphate of ammonia and nickel, the whole of the cobalt, an extremely fmall quantity excepted, will be feparated; but after this there ftill remains fome copper mixed with the falt. I have already announced that this metal may be feparated from the nickel by fubliming the latter with fal ammoniac; but at that time I had never obtained pure nickel. With the compound falt of nickel and ammonia

Cobalt feparated from fulphate of ammonia and nickel by repeated cryftallization.

Copper, by fubliming with fal ammoniac.

* Tranflated from the *Journal de Chimie* of Van Mons, vol. VI. p. 183, March, 1805; and abridged by Van Mons from the *Allgemeines Journal der Chemie*, 1804, vol. III. p. 244.

 a little

Arfenic and iron left.

a little arfenic ftill remains; and there may be iron likewife, when we have been a little too fparing in the addition of nitric acid to the fulphuric folution of cobalt containing calx of nickel.

The triple falt decompofed by carbonate of potafh.

I endeavoured to feparate thefe extraneous metals in the humid way, but not with complete fuccefs. I decompofed by means of carbonate of potafh the triple ammoniacal falt of nickel, free from iron, and as much as poffible from cobalt: the precipitate ftill was vifibly of a greenifh blue. Having edulcorated it, and heated it to rednefs, it changed its colour, as it loft its carbonic acid, to a blackifh gray, which however inclined evidently to a green. The water of edulcoration, which had a greenifh appearance, was evaporated to drynefs, and the refiduum, after being heated red hot, was wafhed again. A green powder remained, which did not lofe its colour in the fire, and confifted in great part of arfeniate of nickel.

The metal reduced.

I mixed each of the two refiduums feparately with a fifth part of charcoal, and expofed them to the heat of a potter's furnace for eighteen hours in a cupel with a little porcelain glaze. The metallic buttons obtained differed a little from each other. Each endured a few blows with a hammer without cracking; but that of the latter refiduum was much more white and fragile than that of the former, the colour of which approached that of fteel and was flightly reddifh. They were both attacked with avidity by nitric acid, and they were attracted by the magnet, but the former only weakly.

The refult not pure nickel.

As from feveral effects on porcelain it appeared to me probable, that pure nickel was a noble metal, I diffolved afrefh in nitric acid the whole quantity reduced, which amounted to feveral ounces, and evaporated the folution to drynefs. I then poured water on the faline mafs, and a beautiful green folution was formed; but a greenifh white refiduum remained, in which I eafily detected the prefence of iron, nickel, and arfenic acid.

The folution precipitated and expofed to a ftrong heat.

The folution, which befide arfenic, contained a confiderable portion of copper, was precipitated by carbonate of potafh, and the refiduum, the colour of which was ftill very lively, though not fo green as that of carbonate of copper, was well wafhed and expofed to a white heat. This changed its apple green colour to a deep green inclining to gray and brown. With a ftronger heat the mafs affumed a grayer brown, and at the

the ſame time appeared to coagulate. There were likewiſe portions of reduced metal in it, that could not be miſtaken.—I could not, however, accompliſh its fuſion in a wind-furnace, ſurmounted with a cupellating furnace dome, and having a long chimney. In conſequence, I divided it into ſeveral portions, which I expoſed in crucibles to the ſtrongeſt heat of a potter's furnace, in which capſules of the moſt refractory clay are frequently ſoftened. Very refractory.

In thoſe crucibles which were placed where the porcelain is longeſt taking, the matter had experienced no change but a coagulation. In the other crucibles it had entered into complete fuſion, yet not into a liquid fuſion, and the crucibles had partly experienced the ſame effect. Here and there in the melted maſs metallic globules were found, the largeſt of which were the ſize of a ſmall nut, and the leaſt that of a cherry-ſtone. Their brilliancy was a mean between that of ſilver and that of Engliſh tin. The ſcoriæ were greeniſh brown, mixed with an amethyſt colour, and in ſome places a deep blue entirely like fuſed oxide of cobalt. The brown colour aroſe from the oxide of copper, which was completely vitrified, and the blue from that of cobalt. The green, on the contrary, proceeded from arſeniate of nickel, which, as I have learned by experience, ſtrongly reſiſts fuſion, without the addition of ſome combuſtible ſubſtance. Fuſed in the ſtrongeſt heat of a potter's furnace. Scoriæ. Arſeniate of nickel.

I attempted to hammer the metallic globules on an anvil, and to my great ſatisfaction I found that they poſſeſſed conſiderable malleability. They were eagerly attracted by the magnet. The metal malleable, and magnetic.

As it was impoſſible to ſeparate the ſcorie with the hammer from the little globules to which they adhered, I collected them together by trituration and decantation, and expoſed them to fuſion afreſh. It was again complete only in the places of the furnace moſt heated. Re-fuſion.

Convinced from theſe reſults, that nickel is reducible in the fire without the addition of any combuſtible matter, I attempted to reduce ſome oxide of this metal, obtained by the decompoſition of the triple ammoniacal ſalt of nickel, which during an uninterrupted labour of eighteen months, I had procured in a very large quantity. On this occaſion the ſame phenomena occurred as in the preceding reductions. Reduced without any addition.

I repeated the melting till the metal had undergone a complete fuſion, and was found collected together in a button at the A button of an ounce and half procured.

the bottom of the crucible. In one crucible which had been exposed to the strongest heat, I obtained a button that weighed an ounce and a half.

Best reduced alone.

I was less successful in my fusion when I mixed the oxide of nickel with porcelain glaze, or when I simply covered it with this glaze; so that I was convinced the best process was to reduce the oxide of nickel directly.

After much time and patience, I succeeded in obtaining several ounces of nickel, which I must consider as absolutely pure, and I shall now proceed to describe the principal characters that I have perceived in it in this state.—To begin with the external characters.

External character of pure nickel. Colour. Unchangeable in the air. Malleable.

The colour of pure nickel is a mean between silver and tin. It undergoes no alteration either from the air of the atmosphere or from the water in it: in other words it is insusceptible of being oxided by the air.

It is perfectly malleable; as it may not only be forged into bars when red hot, but hammered on the anvil while cold into very thin plates. This character removes nickel from the class of semi-metals to that of perfect metals.

Specific gravity.

Its density or specific gravity is pretty considerable: from repeated experiments with my hidrometer cast nickel weighs 8.279, and forged nickel 8.666.

Ductile.

Its tenacity likewise appears considerable, to judge from its great ductility. A piece of cast nickel, weighing five drams allowed itself to be flattened, but not without cracking, into a plate of 13 square inches Rhynland measure, which gives less than $\frac{1}{100}$ of an inch for its thickness. It might probably be drawn into a wire of the same tenuity.

Refractory.

The resistance of nickel to fusion is very considerable, and equals, if it do not surpass, that of manganese. The reader may have observed, from my attempts to fuse it, how difficult it is to obtain any thing decisive on this head.

A noble metal.

At a temperature sufficiently high the pure oxide of nickel is reducible without the addition of any combustible matter. Its great resistance to fusion is the only cause why this reduction presents so many difficulties. Very little oxidation too is perceptible on keeping this metal in a state of incandescence: it is merely tarnished a little more than platina, gold, or silver. Nickel therefore belongs not to the class of perfect metals merely, but to that of noble metals.

Magnetic.

The action of the magnet on nickel is not only very considerable

derable, and little inferior to that on iron; but nickel becomes itself magnetical, or acquires polarity, by the touch, and even in part by striking it with a hammer, or filing it, with the precautions suitable for producing this effect. I discovered the latter property by presenting to the magnet a slip of forged nickel; when, notwithstanding it was polished by the file, it adhered more feebly to the magnet than other slips less polished; but on my presenting its other extremity to the magnet, it adhered to it with great force. It likewise attracted by either side not only iron needles, but plates of nickel half an inch square, which it caused to move about on a smooth table.

Its magnetic property weakened by copper, destroyed by arsenic. Magnetic and malleable in proportion to its purity.

The property which nickel possesses of becoming magnetic is not destroyed, though weakened by its alloy with copper; but arsenic destroys it completely. I had frequent opportunities of making this observation in the course of my experiments. Some nickel, from which I had separated the iron* and the arsenic in the humid way, and which I had afterwards reduced with the addition of a combustible substance, was malleable, and attracted the magnet, though not so forcibly as pure nickel. The same metal, purified with less care, was less malleable, and proportionally less attractable by the magnet. Repeated exposure of the metal to the most powerful heat of a porcelain furnace did not in the least restore to it this property.— Some experiments, which I shall hereafter relate, have convinced me, that copper cannot be entirely separated from nickel in the humid way, and that the only means of separating them is to reduce the cupreous oxide of nickel by fire.

Copper must be separated by fire.

Action of the acids upon nickel.

The sulphuric and muriatic acids have little action upon nickel. The oxide of this metal by the air does not dissolve even in the latter of these acids without the assistance of a strong ebullition. The most appropriate solvents of nickel are the nitric and nitro-muriatic acids. I have already mentioned, that impure nickel, particularly the cupreous is attacked by the nitric acid with heat and vivacity. The action of the same acid on pure nickel is a little different, and particularly on the

Best mode of freeing it from iron.

* The separation of the iron succeeds best by a rapid evaporation of the nitric solution of the ferruginous nickel, by which the iron is precipitated in the form of an insoluble oxide. At the same time a little arsenic is separated in union with the iron. It is preferable, however, to separate the arsenic first, which is effected by the help of a nitric solution of lead. The lead is afterward to be precipitated by a solution of sulphate of potash.

hammered

hammered metal. I have poured nitric acid on nickel both in buttons and laminated, expecting a very active solution; but it has proceeded slowly, and I have even been obliged to have recourse to the heat of a spirit lamp to accelerate it. The dissolution however having appeared to cease, I decanted the liquid and poured on the residuum a fresh quantity of acid of the same strength as the preceding, when on a sudden such a brisk action came on, accompanied with the evolution of heat *, that I could not remove the capsule to the fire-place quickly enough.

I shall now go on to consider some of the characters of pure nickel in the state of oxidation.

Characters of oxide of nickel.

The nitric solution of pure nickel has a beautiful grass-green colour. Carbonate of potash separates from it a pale apple green precipitate. This precipitate well washed and dried is very light. A thousand parts of metallic nickel reduced to this precipitate weigh 2,927 parts.

Precipitate very light.

If this precipitate be exposed to a white heat it becomes of a blackish gray, scarcely inclining to green, and weighing only 1,285. On continuing the fire, the mass approaches the metallic state more and more, and becomes magnetic. This is effected much more speedily if the oxide be moistened with a little oil.

Oil promotes its reduction.

Precipitate by ammonia.

On adding caustic ammonia in excess to a nitric solution of nickel, a precipitate is formed, resembling in colour ammoniure of copper, but not so deep. This colour sometimes changes in a couple of hours to an amethyst red, and to a violet, which colours are converted into an apple-green on the addition of an acid, and again to a blue and violet on the addition of ammonia. If however we add to the solution of nickel a solution of copper, so as to produce no perceptible change, the colour of the precipitate formed by ammonia ceases to assume a red tinge, and the red colour of the ammoniure of nickel disappears on the addition of a little ammoniure of copper; whence it follows, that every precipitate of nickel by ammonia, which retains its blue colour, has copper combined with it.

Its being a noble metal questioned.

* From this it is difficult to believe that nickel, under favourable circumstances, would not become oxided by the combined influence of air and fire. VAN MONS.

On

III.

On the Analysis of Soils, as connected with their Improvement. By HUMPHREY DAVY, Esq. F. R. S. Professor of Chemistry to the Board of Agriculture and to the Royal Institution.

I. Utility of Investigation relating to the Analysis of Soils.

THE methods of improving lands are immediately connected with the knowledge of the chemical nature of soils, and experiments on their composition appear capable of many useful applications.

The importance of this subject has been already felt by some very able cultivators of science; many useful facts and observations with regard to it have been furnished by Mr. Young; it has been examined by Lord Dundonald, in his treatise on the connexion of Chemistry with Agriculture, and by Mr. Kirwan in his excellent essay on Manures: but the enquiry is still far from being exhausted, and new methods of elucidating it are almost continually offered, in consequence of the rapid progress of chemical discovery. Analysis of soils; attended to by Mr. Young, Lord Dundonald, and Mr. Kirwan.

In the following pages I shall have the honour of laying before the Board, an account of those methods of analysing soils which appear most precise and simple, and most likely to to be useful to the practical farmer; they are founded partly upon the labours of the gentlemen, whose names have been just mentioned, and partly upon some later improvements.

II. Of the Substances found in Soils.

The substances which are found in soils, are certain mixtures or combinations of some of the primitive earths, animal and vegetable matter in a decomposing state, certain saline compounds, and the oxide of iron. These bodies always retain water, and exist in very different proportions in different lands; and the end of analytical experiments is the detection of their quantities and mode of union. Soils contain earths, animal and vegetable remains, saline compounds, and oxide of iron.

The *earths* found in common soils are principally silex or the earth of flints, alumine or the pure matter of clay, lime, or calcareous earth, and magnesia.

Silex, or the earth of flints, when perfectly pure, appears in the form of a white powder, which is incombustible, infusible, insoluble Silex.

insoluble in water, and not acted upon by common acids; it is the substance which constitutes the principal part of rock chrystal; it composes a considerable part of hard gravelly soils, of hard sandy soils, and of hard stony lands.

Alumine. *Alumine*, or pure clay, in its perfect state is white like silex; it adheres strongly to the tongue, is incombustible, insoluble in water, but soluble in acids, and in fixed alkaline menstrua. It abounds most in clayey soils and clayey loams; but even in the smallest particles of these soils it is usually united to silex and oxide of iron.

Lime. *Lime* is the substance well known in its pure state under the name of quicklime. It always exists in soils in combination, and that principally with fixed air or carbonic acid, when it is called carbonate of lime; a substance which in the most compact form constitutes marble, and in its looser form chalk. Lime, when combined with sulphuric acid (oil of vitriol), produces sulphate of lime (gypsum), and with phosphoric acid, phosphate of lime. The carbonate of lime, mixed with other substances, composes chalky soils and marles, and it is found in soft sandy soils.

Magnesia. *Magnesia*, when pure, appears as white, and in a lighter powder, than any of the other earths; it is soluble in acid, but not in alkaline menstrua; it is rarely found in soils; when it does exist, it is either in combination with carbonic acid, or with silex and alumine.

Animal decomposing matter. *Animal decomposing matter* exists in very different states, according as the substances from which it is produced are different; it contains much carbonaceous substance, and may be principally resolved by heat into this substance, volatile alkali, inflammable aeriform products, and carbonic acid; it is principally found in lands that have been lately manured.

Vegetable decomposing matter. *Vegetable decomposing matter* is likewise very various in kind, it contains usually more carbonaceous substance than animal matter, and differs from it in the results of its decomposition principally in not producing volatile alkali; it forms a great proportion of all peats; it abounds in rich mould, and is found in larger or smaller quantities in all lands.

Saline compounds. The *saline compounds* found in soils are very few, and in quantities so small, that they are rarely to be discovered. They are principally muriate of soda (common salt), sulphate of magnesia (Epsom salt), and muriate and sulphate of potash, nitrate of lime, and the mild alkalies.

The

The *oxide of iron* is the same with the rust produced by exposing iron to the air and water; it is found in all soils, but is most abundant in yellow and red clays, and in yellow and red siliceous sands. Oxide of iron.

A more minute account of these different substances would be incompatible with the object of this paper. A full description of their properties and agencies may be found in the elementary books on chemistry, and particularly in the System of Chemistry by Dr. Thomson (2d Ed.); and in Henry's Epitome of Chemistry.

III. *Instruments required for the Analysis of Soils.*

The really important instruments required for the analysis of soils are few, and but little expensive. They are a balance capable of containing a quarter of a pound of common soil, and capable of turning when loaded, with a grain; a series of weights from a quarter of a pound Troy to a grain; a wire sieve, sufficiently coarse to admit a pepper corn through its apertures; an Argand lamp and stand; some glass bottles; Hessian crucibles; porcelain, or queen's ware evaporating basons; a Wedgewood pestle and mortar; some filters made of half a sheet of blotting paper, folded so as to contain a pint of liquid, and greased at the edges; a bone knife, and an apparatus for collecting and measuring aeriform fluids. Instruments for analysis. A balance, weights, sieve, lamp, bottles, crucibles, basons, p. and mortar, filters, knife, app. for gases.

The chemical substances or reagents required for separating the constituent parts of the soil, are muriatic acid (spirit of salt), sulphuric acid, pure volatile alkali dissolved in water, solution of prussiate of potash, soap lye, solution of carbonate of ammoniac, of muriate of ammonia, solution of neutral carbonate of potash, and nitrate of ammoniac. An account of the nature of these bodies, and their effects, may be found in the chemical works already noticed; and the reagents are sold together with the instruments mentioned above, by Mr. Knight, Foster Lane, Cheapside, arranged in an appropriate chest. Re-agents. Mur. and sulp. acids, vol. alk. pr. potash, soap lye, carb. amm. mur. amm. carb. pot. nitr. amm.

IV. *Mode of collecting Soils for Analysis.*

In cases when the general nature of the soil of a field is to be ascertained, specimens of it should be taken from different places, two or three inches below the surface, and examined as to the similarity of their properties. It sometimes happens, that upon plains the whole of the upper stratum of the land is How samples of soils are to be collected.

of the same kind, and in this case, one analysis will be sufficient; but in vallies, and near the beds of rivers, there are very great differences, and it now and then occurs that one part of a field is calcareous, and another part siliceous; and in this case, and in analogous cases, the portions different from each other should be separately submitted to experiment.

and preserved if needful.

Soils when collected, if they cannot be immediately examined, should be preserved in phials quite filled with them, and closed with ground glass stoppers.

The quantity of soil most convenient for a perfect analysis, is from two or four hundred grains. It should be collected in dry weather, and exposed to the atmosphere till it becomes dry to the touch.

The specific gravity

The specific gravity of a soil, or the relation of its weight to that of water, may be ascertained by introducing into a phial, which will contain a known quantity of water, equal volumes of water and of soil, and this may be easily done by pouring in water till it is half full, and then adding the soil till the fluid rises to the mouth; the difference between the weight of the soil and that of the water, will give the result. Thus if the bottle contains four hundred grains of water, and gains two hundred grains when half filled with water and half with soil, the specific gravity of the soil will be 2, that is, it will be twice as heavy as water, and if it gained one hundred and sixty-five grains, its specific gravity would be 1825, water being 1000.

is of importance to be known.

It is of importance, that the specific gravity of a soil should be known, as it affords an indication of the quantity of animal and vegetable matter it contains; these substances being always most abundant in the lighter soils.

Other physical properties.

The other physical properties of soils should likewise be examined before the analysis is made, as they denote, to a certain extent, their composition, and serve as guides in directing the experiments. Thus siliceous soils are generally rough to the touch, and scratch glass when rubbed upon it; aluminous soils adhere strongly to the tongue, and emit a strong earthy smell when breathed on; and calcareous soils are soft, and much less adhesive than aluminous soils.

V. Mode

V. *Mode of ascertaining the Quantity of Water of Absorption in Soils.*

Evaporation of the absorbed water, to shew its quantity in soils.

Soils, though as dry as they can be made by continued exposure to air, in all cases still contain a considerable quantity of water, which adheres with great obstinacy to the earths and animal and vegetable matter, and can only be driven off from them by a considerable degree of heat. The first process of analysis is, to free the given weight of soil from as much of this water as possible, without in other respects, affecting its composition; and this may be done by heating it for ten or twelve minutes over an Argand's lamp, in a bason of porcelain, to a temperature equal to 300 * Fahrenheit; and in case a thermometer is not used, the proper degree may be easily ascertained, by keeping a piece of wood in contact with the bottom of the dish; as long as the colour of the wood remains unaltered, the heat is not too high; but when the wood begins to be charred, the process must be stopped. A small quantity of water will perhaps remain in the soil even after this operation, but it always affords useful comparative results; and if a higher temperature were employed, the vegetable or animal matter would undergo decomposition, and in consequence the experiment be wholly unsatisfactory.

One eighth is an extreme proportion.

The loss of weight in the process should be carefully noted, and when in four hundred grains of soil it reaches as high as 50, the soil may be considered as in the greatest degree absorbent, and retentive of water, and will generally be found to contain a large proportion of aluminous earth. When the loss is only from 20 to 10, the land may be considered as only slightly absorbent and retentive, and the silicious earth as most abundant.

VI. *Of the Separation of Stones, Gravel, and vegetable Fibres from Soils.*

Stones, &c. to be separated after the drying, &c.

None of the loose stones, gravel, or large vegetable fibres should be divided from the pure soil till after the water is drawn off; for these bodies are themselves often highly absorbent and retentive, and in consequence influence the fertility of the land.

* In several experiments, in which this process has been carried on by distillation, I have found the water that came over pure, and no sensible quantity of other volatile matter was produced.

The

The next procefs, however, after that of heating, fhould be their feparation, which may be eafily accomplifhed by the fieve, after the foil has been gently bruifed in a mortar. The weights of the vegetable fibres or wood, and of the gravel and ftones fhould be feparately noted down, and the nature of the laft afcertained; if calcareous, they will effervefce with acids; if filiceous, they will be fufficiently hard to fcratch glafs; and if of the common aluminous clafs of ftones, they will be foft, eafily fcratched with a knife, and incapable of effervefcing wiih acids.

VII. *Separation of the Sand and Clay, or Loam, from each other.*

Sand, clay, and loam feparated from each other by elutriation.

The greater number of foils, befides gravel and ftones, contain larger or fmaller proportions of fand of different degrees of finenefs; and it is a neceffary operation, the next in the procefs of analyfis, to detach them from the parts in a ftate of more minute divifion, fuch as clay, loam, marle, and vegetable and animal matter. This may be effected in a way fufficiently accurate, by agitation of the foil in water. In this cafe, the coarfe fand will generally feparate in a minute, and the finer in two or three minutes, whilft the minutely divided earthy, animal, or vegetable matter will remain in a ftate of mechanical fufpenfion for a much longer time; fo that by pouring the water from the bottom of the veffel, after one, two, or three minutes, the fand will be principally feparated from the other fubftances, which, with the water containing them, muft be poured into a filter, and after the water has paffed through, collected, dried and weighed. The fand muft likewife be weighed, and their refpective quantities noted down. The water of lixiviation muft be preferved, as it will be found to contain the faline matter, and the foluble animal or vegetable matters, if any exift in the foil.

VIII. *Examination of the Sand.*

The fand feparated into filiceous and calcareous.

By the procefs of wafhing and filtration, the foil is feparated into two portions, the moft important of which is generally the finely divided matter. A minute analyfis of the fand is feldom or never neceffary, and its nature may be detected in the fame manner as that of the ftones or gravel. It is always either filicious fand, or calcareous fand, or a mixture of both. If it confifts

consists wholly of carbonate of lime, it will be rapidly soluble in muriatic acid, with effervescence; but if it consist partly of this substance, and partly of siliceous matter, the respective quantities may be ascertained by weighing the residuum after the action of the acid, which must be applied till the mixture has acquired a sour taste, and has ceased to effervesce. This residuum is the silicious part: it must be washed, dried, and heated strongly in a crucible; the difference between the weight of it and the weight of the whole, indicates the proportion of calcareous sand.

IX. *Examination of the finely divided Matter of Soils, and Mode of detecting mild Lime and Magnesia.*

The finely divided matter treated.

The finely divided matter of the soil is usually very compound in its nature; it sometimes contains all the four primitive earths of soils, as well as animal and vegetable matter; and to ascertain the proportions of these with tolerable accuracy, is the most difficult part of the subject.

Muriatic acid takes up lime, magnesia, or iron.

The first process to be performed, in this part of the analysis, is the exposure of the fine matter of the soil to the action of the muriatic acid. This substance should be poured upon the earthy matter in an evaporating bason, in a quantity equal to twice the weight of the earthy matter; but diluted with double its volume of water. The mixture should be often stirred, and suffered to remain for an hour or an hour and a half before it is examined.

If any carbonate of lime or of magnesia exist in the soil, they will have been dissolved in this time by the acid, which sometimes takes up likewise a little oxide of iron; but very seldom any alumine.

Precip. of iron (if present) by pruss. alkali;

The fluid should be passed through a filter; the solid matter collected, washed with rain water, dried at a moderate heat, and weighed. Its loss will denote the quantity of solid matter taken up. The washings must be added to the solution, which if not sour to the taste, must be made so by the addition of fresh acid, when a little solution of common prussiate of potash must be mixed with the whole. If a blue precipitate occurs, it denotes the presence of oxide of iron, and the solution of the prussiate must be dropped in till no farther effect is produced. To ascertain its quantity, it must be collected in the same manner as other solid precipitates, and heated red; the result is oxide of iron.

Into

and earth, by carbonate of potash.

Into the fluid freed from oxide of iron, a solution of neutralized carbonate of potash must be poured till all effervescence ceases in it, and till its taste and smell indicate a considerable excess of alkaline salt.

The precipitate that falls down is carbonate of lime; it must be collected on the filter, and dried at a heat below that of redness.

The remaining fluid must be boiled for a quarter of an hour, when the magnesia, if any exist, will be precipitated from it, combined with carbonic acid, and its quantity is to be ascertained in the same manner as that of the carbonate of lime.

Alumine if taken up.

If any minute proportion of alumine should, from peculiar circumstances, be dissolved by the acid, it will be found in the precipitate with the carbonate of lime, and it may be separated from it by boiling for a few minutes with soap lye, sufficient to cover the solid matter. This substance dissolves alumine, without acting upon carbonate of lime.

Carbonate of lime if in plenty, may be estimated by the quantity of carbonic acid.

Should the finely divided soil be sufficiently calcareous to effervesce very strongly with acids, a very simple method may be adopted for ascertaining the quantity of carbonate of lime, and one sufficiently accurate in all common cases.

Carbonate of lime, in all its states, contains a determinate proportion of carbonic acid, *i. e.* about 45 per cent. so that when the quantity of this elastic fluid, given out by any soil during the solution of its calcareous matter in an acid is known, either in weight or measure, the quantity of carbonate of lime may be easily discovered.

When the process by diminution of weight is employed, two parts of the acid and one part of the matter of the soil must be weighed in two separate bottles, and very slowly mixed together till the effervescence ceases; the difference between their weight before and after the experiment, denotes the quantity of carbonic acid lost; for every four grains and a half of which, ten grains of carbonate of lime must be estimated.

The best method of collecting the carbonic acid, so as to discover its volume, is by the pneumatic apparatus, the construction and application of which is described at the end of this paper. The estimation is, for every ounce measure of carbonic acid, two grains of carbonate of lime.

X. Mode

X. *Mode of ascertaining the Quantity of insoluble finely divided Animal and Vegetable Matter.*

Ignition in an open vessel destroys vegetable and animal matters.

After the fine matter of the soil has been acted upon by muriatic acid, the next process is to ascertain the quantity of finely divided insoluble animal and vegetable matter that it contains.

This may be done with sufficient precision, by heating it to strong ignition in a crucible over a common fire till no blackness remains in the mass. It should be often stirred with a metallic wire, so as to expose new surfaces continually to the air; the loss of weight that it undergoes denotes the quantity of the substance that it contains destructible by fire and air.

The smell shews whether it be animal or vegetable.

It is not possible to ascertain whether this substance is wholly animal or vegetable matter, or a mixture of both. When the smell emitted during the incineration is similar to that of burnt feathers, it is a certain indication of some animal matter; and a copious blue flame at the time of ignition, almost always denotes a considerable proportion of vegetable matter. In cases when the experiment is needed to be very quickly performed, the destruction of the decomposible substances may be assisted by the agency of nitrate of ammoniac, which at the time of ignition may be thrown gradually upon the heated mass in the quantity of twenty grains for every hundred of residual soil. It affords the principle necessary to the combustion of the animal and vegetable matter, which it causes to be converted into elastic fluids; and it is itself at the same time decomposed and lost.

XI. *Mode of separating aluminous and silicious Matter and Oxide of Iron.*

The residual silex, alumine and oxide of iron separated.

The substances remaining after the decomposition of the vegetable and animal matter, are generally minute particles of earthy matter, containing usually alumine and silex with combined oxide of iron.

Dilute sulphuric acid takes up the two first.

To separate these from each other, the solid matter should be boiled for two or three hours with sulphuric acid, diluted with four times its weight of water; the quantity of the acid should be regulated by the quantity of solid residuum to be acted on, allowing for every hundred grains two drachms or one hundred and twenty grains of acid.

The

The substance remaining after the action of the acid, may be considered as silicious; and it must be separated and its weight ascertained, after washing and drying in the usual manner.

Carbonate of ammonia throws down the alumine.

The alumine and the oxide of iron, if they exist, are both dissolved by the sulphuric acid; they may be separated by carbonate of ammoniac, added to excess; it throws down the alumine, and leaves the oxide of iron in solution, and this substance may be separated from the liquid by boiling.

Should any magnesia and lime have escaped solution in the muriatic acid, they will be found in the sulphuric acid; this, however, is scarcely ever the case; but the process for detecting them and ascertaining their quantities, is the same in both instances.

More accurate process.

The method of analysis by sulphuric acid, is sufficiently precise for all usual experiments; but if very great accuracy be an object, dry carbonate of potash must be employed as the agent, and the residuum of the incineration must be heated red for half an hour, with four times its weight of this substance, in a crucible of silver, or of well baked porcelain. The mass obtained must be dissolved in muriatic acid, and the solution evaporated till it is nearly solid; distilled water must then be added, by which the oxide of iron and all the earths, except silex, will be dissolved in combination as muriates. The silex, after the usual process of lixiviation, must be heated red; the other substances may be separated in the same manner as from the muriatic and sulphuric solutions.

This process is the one usually employed by chemical philosophers for the analysis of stones.

XII. *Mode of discovering soluble Animal and Vegetable Matter, and Saline Matter.*

Matters soluble in water.

If any saline matter, or soluble vegetable or animal matter, is suspected in the soil, it will be found in the water of lixiviation used for separating the sand.

This water must be evaporated to dryness in an appropriate dish, at a heat below its boiling point.

If the solid matter obtained is of a brown colour and inflammable, it may be considered as partly vegetable extract. If its smell, when exposed to heat, be strong and fetid, it contains animal mucilaginous or gelatinous substance; if it be white

white and transparent, it may be considered as principally saline matter. Nitrate of potash (nitre) or nitrate of lime, is indicated in this saline matter, by its scintillating with a burning coal. Sulphate of magnesia may be detected by its bitter taste; and sulphate of potash produces no alteration in solution of carbonate of ammoniac, but precipitates solution of muriate of barytes.

XIII. *Mode of detecting Sulphate of Lime (Gypsum) and Phosphate of Lime in Soils.*

Should sulphate of phosphate of lime be suspected in the entire soil, the detection of them requires a particular process upon it. A given weight of it, for instance four hundred grains, must be heated red for half an hour in a crucible, mixed with one-third of powdered charcoal. The mixture must be boiled for a quarter of an hour, in a half pint of water, and the fluid collected through the filter, and exposed for some days to the atmosphere in an open vessel. If any soluble quantity of sulphate of lime (gypsum) existed in the soil, a white precipitate will gradually form in the fluid, and the weight of it will indicate the proportion. Sulphate of Lime.

Phosphate of lime, if any exist, may be separated from the soil after the process for gypsum. Muriatic acid must be digested upon the soil, in quantity more than sufficient to saturate the soluble earths; the solution must be evaporated, and water poured upon the solid matter. This fluid will dissolve the compounds of earths with the muriatic acid, and leave the phosphate of lime untouched. Phosphate of lime.

It would not fall within the limits assigned to this paper, to detail any processes for the detection of substances which may be accidentally mixed with the matters of soils. Manganese is now and then found in them, and compounds of the barytic earth; but these bodies appear to bear little relation to fertility or barrenness, and the search for them would make the analysis much more complicated without rendering it more useful.

XIV. *Statement of Results and Products.*

When the examination of a soil is compleated, the products should be classed, and their quantities added together, and if they nearly equal the original quantity of soil, the analysis may be considered as accurate. It must, however, be noticed, that Products stated.

that when phosphate or sulphate of lime are discovered by the independent process XIII. a correction must be made for the general process, by subtracting a sum equal to their weight from the quantity of carbonate of lime, obtained by precipitation from the muriatic acid.

In arranging the products, the form should be in the order of the experiments by which they were obtained.

Thus 400 grains of a good silicious sandy soil may be supposed to contain

		Grains.
Of water of absorption - - - -		18
Of loose stones and gravel principally silicious -		42
Of undecompounded vegetable fibres - -		10
Of fine silicious sand - - -		200
Of minutely divided matter separated by filtration and consisting of		
Carbonate of lime - - -	25	
Carbonate of magnesia - - -	4	
Matter destructible by heat, principally vegetable - - - -	10	
Silex - - - - -	40	
Alumine - - - - -	32	
Oxide of iron - - - -	4	
Soluble matter, principally sulphate of potash and vegetable extract - -	5	
Gypsum - - - -	3	
Phosphate of lime - - -	2	
	———	125
Amount of all the products		395
Loss - - -		5

In this instance the loss is supposed small; but in general, in actual experiments, it will be found much greater, in consequence of the difficulty of collecting the whole quantities of the different precipitates; and when it is within thirty for four hundred grains, there is no reason to suspect any want of due precision in the processes.

XV. *This general Method of Analysis may in many Cases be much simplified.*

Simplification, &c. of the analysis.

When the experimenter is become acquainted with the use of the different instruments, the properties of the reagents, and

and the relations between the external and chemical qualities of soils, he will seldom find it necessary to perform, in any one case, all the processes that have been described. When his soil, for instance, contains no notable proportion of calcareous matter, the action of the muriatic acid IX. may be omitted. In examining peat soils, he will principally have to attend to the operation by fire and air X.; and in the analysis of chalks and loams, he will often be able to omit the experiment by sulphuric acid XI.

In the first trials that are made by persons unacquainted with chemistry, they must not expect much precision of result. Many difficulties will be met with; but in overcoming them, the most useful kind of practical knowledge will be obtained; and nothing is so instructive in experimental science, as the detection of mistakes. The correct analyst ought to be well grounded in chemical information; but perhaps there is no better mode of gaining it, than that of attempting original investigations. In pursuing his experiments, he will be continually obliged to learn from books, the history of the substances he is employing or acting upon; and his theoretical ideas will be more valuable in being connected with practical operation, and acquired for the purpose of discovery.

XVI. *On the Improvement of Soils, as connected with the Principle of their Composition.*

Improvement of lands from the known composition of fertile or sterile soils.

In cases when a barren soil is examined with a view to its improvement, it ought in all cases, if possible, to be compared with an extremely fertile soil in the same neighbourhood, and in a similar situation: the difference given by their analyses would indicate the methods of cultivation; and thus the plan of improvement would be founded upon accurate scientific principles.

If the fertile soil contained a large quantity of sand, in proportion to the barren soil, the process of amelioration would depend simply upon a supply of this substance; and the method would be equally simple with regard to soils deficient in clay or calcareous matter.

In the application of clay, sand, loam, marle, or chalk to lands, there are no particular chemical principles to be observed; but when quick lime is used, great care must be taken that it is not obtained from the magnesian limestone; for in this

this case, as has been shewn by Mr. Tennant, it is exceedingly injurious to land *. The magnesian limestone may be distinguished from the common limestone by its greater hardness, and by the length of time that it requires for its solution in acids, and it may be analysed by the process for carbonate of lime and magnesia IX.

When the analytical comparison indicates an excess of vegetable matter, as the cause of sterility, it may be destroyed by much pulverization and exposure to air, by paring and burning, or the agency of lately made quicklime. And the defect of animal and vegetable matter must be supplied by animal or vegetable manure.

XVII. *Sterile Soils in different Climates and Situations must differ in Composition.*

Different climates and local circumstances require different compounds for fertile soils.

The general indications of fertility and barrenness, as found by chemical experiments, must necessarily differ in different climates, and under different circumstances. The power of soils to absorb moisture, a principle essential to their productiveness, ought to be much greater in warm and dry countries, than in cold and moist ones; and the quantity of fine aluminous earth they contain larger. Soils likewise that are situated on declivities, ought to be more absorbent than those in the same climate on plains or in valleys †. The productiveness of soils must likewise be influenced by the nature of the subsoil, or the earthy or stony strata on which they rest; and this circumstance ought to be particularly attended to, in considering their chemical nature, and the system of improvement. Thus a sandy soil may sometimes owe its fertility to the power of the subsoil to retain water; and an absorbent clayey soil may occasionally be prevented from being barren, in a moist climate, by the influence of a substratum of sand or gravel.

XVIII. *Of the chemical Composition of fertile Corn Soils in the Climate.*

Actual composition of some fertile soils.

Those soils that are most productive of corn, contain always certain proportions of aluminous and calcareous earth in a finely divided state, and a certain quantity of vegetable or animal matter.

* Phil. Transactions for 1799, p. 305. This limestone is found abundantly in Yorkshire, Derbyshire, and Somersetshire.

† Kirwan. Transf. Irish Academy, Vol. V. p. 175.

The

The quantity of calcareous earth is however very various, and in some cases exceedingly small. A very fertile corn soil from Ormiston in East Lothian afforded me in an hundred parts, only eleven parts of mild calcareous earth; it contained twenty-five parts of silicious sand; the finely divided clay amounted to forty-five parts. It lost nine in decomposed animal and vegetable matter, and four in water, and afforded indications of a small quantity of phosphate of lime.

This soil was of a very fine texture, and contained very few stones or vegetable fibres. It is not unlikely that its fertility was in some measure connected with the phosphate; for this substance is found in wheat, oats, and barley, and may be a part of their food.

A soil from the low lands of Somersetshire, celebrated for producing excellent crops of wheat and beans without manure, I found to consist of one-ninth of sand, chiefly silicious, and eight-ninths of calcareous marle tinged with iron, and containing about five parts in the hundred of vegetable matter. I could not detect in it any phosphate or sulphate of lime, so that its fertility must have depended principally upon its power of attracting principles of vegetable nourishment from water and the atmosphere *.

Mr. Tillet, in some experiments made on the composition of soils at Paris, found that a soil composed of three-eighths of clay, two-eighths of river sand, and three-eighths of the parings of limestone, was very proper for wheat.

XIX. *Of the Composition of Soils proper for bulbous Roots and for Trees.*

Soils proper for bulbous roots and trees.

In general, bulbous roots require a soil much more sandy, and less absorbent than the grasses. A very good potatoe soil, from Varsel in Cornwall, afforded me seven-eighths of silicious sand; and its absorbent power was so small, that one hundred parts lost only two by drying at 400 Fahrenheit.

Plants and trees, the roots of which are fibrous and hard, and capable of penetrating deep into the earth, will vegetate to advantage in almost all common soils, which are moderately dry, and which do not contain a very great excess of vegetable matter.

* This soil was sent to me by T. Poole, Esq. of Nether Stowey. It is near the opening of the river Parret into the British Channel; but, I am told, is never overflowed.

I found

I found the soil taken from a field at Sheffield-place in Suffex, remarkable for producing flourifhing oaks, to confift of fix parts of fand, and one part of clay and finely divided matter. And one hundred parts of the entire foil fubmitted to analyfis, produced

	Parts.
Water	3
Silex	54
Alumine	28
Carbonate of lime	3
Oxide of iron	5
Decompofing vegetable matter	4
Lofs	3

XX. *Advantages of Improvements made by changing the Compofition of the earthy Parts of Soils.*

Soils rendered fertile by changing the compofition of the earthy parts, are more permanent than manured foils.

From the great difference of the caufes that influence the productivenefs of lands, it is obvious that in the prefent ftate of fcience, no certain fyftem can be devifed from their improvement, independent of experiment; but there are few cafes in which the labour of analytical trials will not be amply repaid by the certainty with which they denote the beft methods of amelioration; and this will particularly happen, when the defect of compofition is found in the proportions of the primitive earths.

In fupplying animal or vegetable manure, a temporary food only is provided for plants, which is in all cafes exhaufted by means of a certain number of crops; but when a foil is rendered of the beft poffible conftitution and texture, with regard to its earthy parts, its fertility may be confidered as permanently eftablifhed. It becomes capable of attracting a very large portion of vegetable nourifhment from the atmofphere, and of producing its crops with comparatively little labour and expence.

Defcription of the Apparatus for the Analyfis of Soils.

Apparatus for experiments.

A. Retort.

B. B. Funnels for the purpofe of filtrating.

D. Balance.

E. Argand's lamp.

F, G, H, K. The different parts of the apparatus required for meafuring the quantity of elaftic fluid given out during the

the action of an acid on calcareous soils. F. Represents the bottle for containing the soil. K. The bottle containing the acid furnished with a stopcock. G. The tube connected with a flaccid bladder. I. The graduated measure. H. The bottle for containing the bladder. When this instrument is used, a given quantity of soil is introduced into F; K is filled with muriatic acid diluted with an equal quantity of water; and the stop-cock being closed is connected with the upper orifice of F, which is ground to receive it. The tube G is introduced into the lower orifice of F, and the bladder connected with it placed in its flaccid state into H, which is filled with water. The graduated measure is placed under the tube of H. When the stop-cock of K is turned, the acid flows into F, and acts upon the soil; the elastic fluid generated passes through G into the bladder, and displaces a quantity of water in H equal to it in bulk, and this water flows through the tube into the graduated measure; the water in which gives by its volume the indication of the proportion of carbonic acid disengaged from the soil; for every ounce measure of which two grains of carbonate of lime may be estimated.

L. Represents the stand for the lamp.

M, N, O, P, Q, R, S. Represent the bottles containing the different reagents.

IV.

Discovery of a new Vegetable Substance, by Mr. Rose *.

A CONCENTRATED decoction of the root of elecampane, *inula helenium*, after standing some hours, deposits a white powder, appearing at first sight much like starch, but differing from it both in its principles and in its manner of comporting itself with other substances.

Decoction of elecampane root deposits a powder resembling starch.

1. This substance is generally insoluble in cold water. Being triturated with it a white milky liquor is formed, which soon deposits a heavy white powder, and leaves the supernatant water clear and limpid.

It is insoluble in cold water.

2. It dissolves very well in boiling water. On heating to ebullition one part of the white powder, with four parts of water,

Soluble in boiling water.

* From Gehlen's Journal for 1804, Vol. III. p. 217.

water, a complete ſolution is obtained, which paſſes through filtering paper while hot, but on cooling acquires a mucilaginous conſiſtence and a dull colour. In the courſe of ſome hours this ſolution depoſits the greater part of the ſubſtance diſſolved in the form of a compact white powder.

But much ſubſides on cooling.

A ſolution of one part of gum arabic, in four parts of water is much thicker, of a more tenacious conſiſtence, and froths lightly, which the ſolution of the powder from the elecampane root does not.

Differs from ſolution of gum-arabic.

3. On mixing the ſolution of the white powder with an equal quantity of alcohol, the mixture is at firſt clear, but in a little time the powder ſeparates in the form of a tumid white ſediment, leaving the fluid above it tranſparent. A ſolution of gum-arabic on the addition of alcohol becomes immediately milky, and long retains this appearance, no kind of powder ſeparating even in ſeveral days.

Alcohol ſeparates it from water,

does not gum-arabic.

4. When thrown on burning coals, the white powder melts like ſugar and evaporates, diffuſing a white, thick, pungent ſmoke, with a ſmell of burnt ſugar. After this combuſtion a light reſiduum only remains, which runs into the coal. Starch emits a ſimilar ſmoke, but does not melt, and leaves a coally reſiduum much greater in quantity. Gum-arabic under the ſame circumſtances gives out ſcarcely any ſmoke.

Melts, emits a thick ſmoke, and leaves little reſiduum.

Thus differs from ſtarch,

and from gum.

Heated in an iron ſpoon over charcoal the powder firſt melts, and emits the ſmoke above deſcribed. As ſoon as the ſpoon becomes red hot, it burns with a vivid light flame, and leaves a very trifling coally reſiduum. Starch under the ſame circumſtances does not melt, is much longer before it burns, and leaves a conſiderable reſiduum of coally matter. Gum-arabic only ſparkles, does not take fire, and leaves a great deal of coal, which is readily convertible into grayiſh aſhes.

On red hot iron burns.

Starch.

Gum.

5. By dry diſtillation we obtain from this powder of the elecampane root a brown empyreumatic acid, having the ſmell of pyroxalic acid, but not an atom of empyreumatic oil.

Dry diſtillation produces an acid, but no oil.

6. The nitric acid transforms the powder only into malic acid and oxalic acid, and when uſed in great exceſs into acetic acid: but we do not obtain an atom of the ſaccholactic acid, which gum-arabic treated in the ſame manner furniſhes ſo abundantly; or of the fatty matter which is generated by the action of nitric acid on ſtarch.

Nitrid acid produces malic, oxalic, and in exceſs acetic.

Gum the ſaccholactic acid.

Starch fat.

2 From

From all these phenomena it follows, that this farinaceous powder extracted from elecampane root, is neither starch nor gum, but a peculiar vegetable substance holding a middle rank between the two. It is probable, that it exists in many other vegetables, and that several products hitherto considered as starch are of the same nature as this farina.

Hence of a nature between gum and starch, and probably exists in other vegetables.

V.

New Galvanic Discoveries by Mr. RITTER, *extracted from a Letter from Mr.* CHRIST. BERNOULLI *.

I HERE transmit you the information you requested respecting the late experiments of Mr. Ritter, to which I subjoin some account of that gentleman.

1. *Charging of a Louis d'Or by the Pile.*

The pile with which Mr. Ritter usually makes his experiments consists of a hundred pairs of metallic plates, two inches in diameter. The pieces of zinc have a rim to prevent the liquid pressed out from flowing away. The apparatus is always insulated by several plates of glass.

Mr. Ritter's common pile.

As Mr. Ritter at present resides in a village near Jene, I have not been able to see his experiments with his grand battery of two thousand pieces, or with his battery of fifty pieces, each thirty-six inches square, the action of which continues very perceptible for a fortnight. Neither have I seen his experiments with the new battery of his invention, consisting of a single metal, and which he calls *the charging pile.*

His grand battery, large battery, and charging pile.

I have frequently however, seen him galvanise louis d'or lent him by persons present. To effect this, he places the louis between two pieces of pasteboard thoroughly wetted, and keeps it six or eight minutes in the chain of circulation connected with the pile. Thus the louis becomes charged, without being immediately in contact with the conducting wires. If this louis be applied afterward to the crural nerves of a frog recently prepared, the usual contractions will be excited.

Louis d'or charged by being kept in the galvanic circuit, excites contractions,

* Translated from the *Journal de Chimie and de Physique* of Van Mons, No. 17, p. 133, March, 1805.

and may thus be distinguished among others, excited. I had put a louis thus galvanised into my pocket, and Mr. Ritter said to me a few minutes after, that I might find out this louis from among the rest, by trying them in succession upon the frog. Accordingly I made the trial, and in reality distinguished among several others a single one, in which the exciting quality was very evident. This charge is retained in proportion to the time that the piece has remained in the circuit of the pile. Of three different louis which Mr. Ritter charged in my presence, neither lost its charge in less than five minutes. All these experiments succeeded completely, and nothing seemed so easy as to repeat them.

as it does not lose its charge for some minutes.

This shows the affinity of the galvanic with the magnetic fluid, between which and the electric, it holds a middle place.

This retention of the galvanic charge by a metal in contact with the hand, and with other metals, shews this communication of the galvanic virtue to have more affinity with magnetism than with electricity, and assigns to the galvanic fluid an intermediate rank between the other two.

Several pieces may be charged at once.

In the manner which I have just described, Mr. Ritter can charge at once as many pieces as he wishes. It is sufficient if the two extreme pieces of the number communicate with the pile through the intervention of wet pasteboards. It is with metallic discs charged in this manner, and placed upon one another with pieces of wet pasteboard alternately interposed, that Mr. Ritter constructs his charging pile, which ought in remembrance of its inventor to be called the *Ritterian pile*. The construction of this pile shows, that each metal galvanised in this way acquires polarity, as the needle does when touched with a magnet. Though I have had no opportunity of seeing this new pile, I have convinced myself of the reality of the phenomenon by an experiment of the highest importance to science, and for the invention of which we are equally indebted to the same ingenious philosopher.

Ritterian pile.

Metals thus charged acquire polarity.

2. *Different Excitability of the Parts of Animals.*

Different excitability of the parts of animals.

During the course of several years in which Mr. Ritter has been employed in galvanic pursuits, and during which he has made many thousands of experiments on the excitation produced in the frog by the contact of two different metals, for Mr. Ritter has not entirely abandoned the original mode of galvanising, like most other experimentalists, who employ Volta's pile exclusively; he had perceived not only a very striking difference in the excitability of the different parts of animals,

animals, but also a difference of excitement between the extenſor and flexor muſcles, according as the poſitive or negative pole was applied to them, or as they were acted upon the inſtant after the metals were brought into contact or ſeparated from each other.

When the excitability is at its higheſt point of energy, as in very young frogs the moment after they are prepared, or in adult frogs during the coupling ſeaſon, the flexors alone contract, and in particular the flexor muſcles of that thigh to which the ſilver or negative metal is applied, contract at the inſtant when the metals come into contact, while thoſe of the thigh to which the zinc or poſitive metal is applied, contract at the inſtant of their ſeparation.

When the excitability of the animal is greateſt, the flexors contract by poſitive galvaniſm:

The oppoſite effects are obſervable in frogs, the excitability of which is on the point of being extinguiſhed, (Ritter's fifth degree.) In this caſe the extenſors only contract, and the flexors remain abſolutely motionleſs. At the moment of contact of the metals the muſcles on the zinc ſide alone are thrown into action, and at the moment of ſeparation thoſe on the ſilver ſide.

when it is loweſt the extenſors contract by negative.

Mr. Ritter diſtinguiſhes three degrees of mean excitability. At the ſecond degree (the firſt of the three mean degrees,) when the metals are brought into contact, a ſtrong excitement of the flexors is diſplayed on the ſilver ſide, and a weak excitement of the extenſors on the zinc ſide; and when the metals are ſeparated a ſtrong excitement of the flexors is ſeen on the zinc ſide, and a weak excitement of the extenſors on the ſilver ſide.

When the excitability is between the medium and either extreme, the effect on the flexors and extenſors ſimultaneous but unequal.

At the fourth degree of excitability the contrary takes place. At the third or middle degree the excitability appears to be equally diſtributed, the contractions on each ſide appear equal, and at the moment of contact the flexors contract on the ſilver ſide, the extenſors on the zinc ſide; while at the moment of ſeparation the extenſors contract on the ſilver ſide, and the flexors on the zinc ſide.

At the medium, equal as well as ſimultaneous.

Mr. Ritter ſhowed me all theſe phenomena, and it was very eaſy to diſtinguiſh the different contractions. I have not yet had time to repeat theſe experiments, but I am afraid, eaſy as they appeared to be, they will require an experienced hand, to produce ſuch diſtinct effects as I ſaw. None of the experiments which Mr. Ritter performed before me ſucceeded with him

The experiments do not always ſucceed on the firſt trial.

Mr. Ritter's merit not sufficiently appreciated,

him the firſt time. Moſt of theſe experiments have never yet been made public, and few philoſophers have juſtly appreciated the value of thoſe which have been given to the world. There are ſome people, who, habituated ſolely to the ſtriking effects of groſſer phyſics, ſuppoſe it impoſſible for a young philoſopher to ſee any thing more than themſelves in the delicate phenomena of a more refined order of phyſical experiments. What has greatly contributed to prevent Mr. Ritter from attaining the high reputation he deſerves is his ſtyle, which, by endeavouring to give it preeiſion, he has rendered obſcure; but in converſation it is quite otherwiſe, as here he combines the ſtricteſt logic with the greateſt ſimplicity of expreſſion.

partly owing to his ſtyle.

Account of Mr. Ritter.

Mr. Ritter is one of thoſe men, who owe every thing to the inſpiration of genius, nothing to education. He was intended for a mechanical occupation, when the diſcoveries of galvani excited in him that innate taſte for the phyſical ſciences, which has carried him over every obſtacle, and raiſed him to rank among the firſt natural philoſophers. Deſtitute of every ſource for procuring himſelf the apparatus indiſpenſable to ordinary phyſics, but ſwayed by the enthuſiaſm of inquiry, he greedily ſeized the opportunity of obeying this impulſe by purſuing a ſeries of experiments, that require only a ſimple and not a very expenſive apparatus. Europe has rung with the ſucceſs he has obtained within the ſeven years he has given to his reſearches. He muſt have written much to procure himſelf a large pile, and the moſt neceſſary books of natural philoſophy.

Not leſs indefatigable as an experimenter than ingenious as a theoriſt, he has committed to writing thouſands of experiments, which his time divided between galvanic experiments, application to other branches of phyſics, and the ſtudy of languages, has not yet allowed him to put in order for publication. But this ſtate of conſtraint is about to be at an end. The elector Bavaria, that enlightened prince, whoſe philoſophical beneficence attracts to his dominions the moſt diſtinguiſhed men of ſcience and learning throughout Europe, has juſt appointed Mr. Ritter a Member of the Academy of Munich, with a ſalary of about 200l. a year.

He is compoſing a ſyſtematic work on galvaniſm.

Mr. Ritter has been employed theſe ſix months in compoſing a ſyſtematic work on galvaniſm, but he does not think he

he shall be able to finish it in less than two or three years. When I left him he was going to publish *Tables of Galvanic Affinity*, including all the substances on which he has made experiments. These tables will be of as much importance to galvanism as those of Bergman were to chemistry: they will show, though not yet in a complete manner, the order in which substances follow each other with respect to exciting or receiving the galvanic action.

Publishing tables of galvanic affinity.

But to return to the experiments respecting the charging of metals. Mr. Ritter, after having shown me his experiments on the different contractibility of various muscles, made me observe, that the piece of gold galvanised by communication exerts at once the action of two metals, or of one constituent part of the pile; and that the half which was next the negative pole while in the circle became positive, and the half toward the positive pole became negative. I was completely convinced of the reality of these different phenomena, so important to physic in general, and to physiology in particular.

The galvanised piece of metal has two poles.

Mr. Ritter having discovered the method of galvanising metals, as iron is rendered magnetic, and having observed that galvanised metals always exhibit two poles, as the magnetic needle does, had the curiosity to observe the effect of golden needles charged with galvanism and balanced on a pivot. To his surprise he perceived, that these needles had a certain dip and variation, and that the angle of variation, the quantity of which I am sorry I cannot recollect, was uniformly the same in all his experiments. It differs however from that of the magnetic needle, and the positive pole always dips.

Golden needles galvanised and suspended, have both dip and variation, but different from the magnetic.

VI.

Improvement in applying the Points in Electrical Machines. By Mr. G. J. Singer.

To Mr. NICHOLSON,

SIR, *Princes Street, Sep. 19th*, 1805.

IN the ordinary construction of electrical machines, the collecting points are fixed, and by the least accidental motion are liable to scratch the glass, to obviate this inconvenience, I place

place my points in a cylindrical wire, terminated by ſmooth wooden balls, whoſe ſemidiameter is leſs than the length of the points: This wire is moveable on its axis, by means of a ſpring ſocket annexed to the ſtem which enters the conductor: The points may of courſe be placed at any required elevation, and the greateſt intenſity any variation in their ſituation produces, be obtained. When, the points are elevated a little above the horizontal line, the danger of ſcratching the glaſs is effectually prevented, by the balls coming in contact while the points are kept at a ſmall diſtance. The ſecurity this application produces, and the additional intenſity it affords, have induced me to trouble you with this communication.

I am,
Dear Sir,
Your's, &c.
G. J. SINGER.

VII.

Queſtion whether Light as a Body may not have its Temperature raiſed or lowered, and produce the Effects aſcribed to reflected Heat. By J. P.

To Mr. NICHOLSON.

SIR,

Queſtion reſpecting light.

POSSESSING no differential thermometer, nor any time to employ it, I cannot prove whether my opinion is well founded or not, reſpecting the ingenious experiments of Mr. Leſlie or of M. Pictet, by which the reflection of inviſible (not radiant) heat, and even of cold, appears to have been proved.

Inſtead of there being an actual reflection of heat as a ſubſtance, or of cold as a ſubſtance, is it not in all theſe caſes a reflection of heated or of cooled light? In the experiments with the heated canniſter, the light of the room is, I doubt not, heated by the canniſter; and if collected in a focus, muſt produce an effect on the thermometer, anſwerable to the increaſed quantity of heat with which it is impregnated. Thus alſo in Monſ. Pictet's experiment, the light intercepted by the mirror and thence reflected, has been deprived of a portion of its caloric, or in other words cooled, by the ice; at the focal

focal point therefore will be a collection of cooled rays of light, which must necessarily occasion an effect on the thermometer, the reverse of that of the former experiment. That light is a body capable of being united with caloric, and that heated or cooled light should thus be reflected and occasion all the phenomena of Mr. Leslie's and of M. Pictet's experiments, appears to be much more probable, than that this calorific and frigorific fluid should be the ambient air, or that cold, as a body, should be reflected from mirrors in such a manner as light is perhaps only capable of being reflected. Were the experiments so made that no light should be in the room, and only a small confined portion of light used to examine the thermometer, these conjectures would be put to the trial, and I trust the mystery would be removed.

Sir, your's,

J. P.

VIII.

*Experiments on a Mineral formerly called false Tungstein, now Cerite, in which a new Metal has been found.**

MR. Klaproth, about eight months ago, says Mr. Vauquelin, sent me word, that he had discovered in the tungstein of Bastnas a new earth, to which he gave the name of ochroit, on account of the red colour it acquired by calcination. Messrs. Hisinger and Berzelius, hearing this, wrote to Mr. V. claiming the priority of discovery, but affirming at the same time, that what they had found was a new metal. These gentlemen afterward sent Mr. V. specimens of the mineral, which he analysed in company with two experienced practical chemists, Messrs. Tassaert and Bergman. The following were the results of their analysis:

Klaproth supposed he had discovered a new earth in the false tungstein; Hisinger and Berzelius a new metal.

The pure cerite † is semitransparent, with a slight rosy tinge,

Characters of pure cerite.

* Abridged from a paper by Vauquelin in the *Annales de Chimie*, Vol. LIV. p. 28, and another by Messrs. Hisinger and Berzelius in van Mons's *Journal de Chimie*, Vol. VI. p. 142.—C.

† Messrs. H. and B. have given to the metal the name of *cerion* or *cerium*, from the new planet Ceres, and to the mineral in which they discovered it that of *cerite*.

or

or of a light or deep flesh-colour *. It is sufficiently hard to scratch glass †, strikes fire with difficulty, and its specific gravity is 4·530. It has no determinate crystalline figure. Its fracture is compact ‡, and a little shining. Its powder is of a greyish colour; it becomes yellow by calcination, and loses twelve per cent §.

Treated with nitro-muriatic acid.

Exp. 1. Two hundred parts of this mineral treated with nitro-muriatic acid three times successively, gave abundance of nitrous acid and oxigenated muriatic acid gas. The first and second solutions being diluted with water were of a gold colour; the third was colourless. The former two being mixed deposited spontaneously in time a small quantity of white sediment. The residuum left by the nitro-muriatic acid was of a gray colour with a slight roseate tinge, and weighed 62, so that 138 parts were dissolved.

The solution precipitated

Exp. 2. The solutions being evaporated to the consistence of syrup to volatilise, the superfluous acid remained clear to the end of the operation. Their residuum, diluted with water, afforded a milky liquor, with a slight rosy tint, and a very astringent taste.

by prussiate of potash and ammonia.

Prussiate of potash produced in it a greenish blue precipitate: the colour of which was changed to a brown by a small quantity of ammonia.

All the liquor into which a small quantity of ammonia had been put to precipitate the iron alone was poured into a filter, but would not pass through. It was heated therefore, and filtered, when it appeared of a gold colour, and had a very saccharine taste. Prussiate of potash and oxalate of ammonia threw down from it perfectly white precipitates.

The matter left on the filter continued for a long time to impart a yellow tinge to the water with which it was washed. It was of a red colour, and appeared like oxide of iron at a maximum of oxidation.

Examined by different reagents.

The solution thus deprived of the red matter by ammonia, was examined by various reagents. Prussiate of potash gave with it a white, flocculent, gelatinous precipitate. Infusion

* Opake, and sometimes but very rarely, yellowish. Messrs. H. and B.

† Does not scratch glass. H. and B.

‡ Unequal and angular. H. and B.

§ Six or seven. H. and B.

of

of galls, a brown, flocculent fediment, unaffected by muriatic acid. Carbonate of potafh, a very copious white gelatinous precipitate. Cauftic potafh, the fame: and an excefs of this reagent produced no change. Oxalate of ammonia, a very copious, white, flocculent precipitate, infoluble in an excefs of oxalic acid. Sulphuric acid, a yellow cryftalline precipitate foluble in water. Muriate of tin whitened the folution without forming any precipitate.

Exp. 3. After this the folution was evaporated, when it inftantly became turbid, and formed an abundant flefh-coloured depofit. This was treated with acidulous oxalate of potafh to diffolve the iron without fuccefs: the addition of nitric acid was as unfuccefsful: but muriatic acid added to the preceding diffolved the precipitate with effervefcence and the emiffion of oxigenated muriatic acid gas. A white cryftalline fubftance however, remained, confifting of oxide of cerium with oxalic acid. The greater part of the excefs of acid in the folution being faturated with ammonia, oxalate of ammonia was added till no more precipitate was formed. This precipitate had all the properties of oxalate of cerium. Ammonia threw down from the filtered liquor oxide of iron. Attempts to free it from iron.

Exp. 4. The matter precipitated from the folution of cerium by ammonia in *Exp.* 2, diffolved with effervefcence in muriatic acid. Oxalate of ammonia threw down from this folution oxide of cerium, and the filtered liquor contained oxide of iron tolerably pure. Muriate precipitated by oxalate of ammonia.

Exp. 5. The liquor freed from the greater part of the iron by ammonia and heat, which had notwithftanding a flight rofeate tinge, was precipitated by oxalate of ammonia. The precipitate at the moment of its formation had the appearance of muriate of filver, but foon became granulous and fubfided in this form. The liquor paffed through the filter colourlefs, and the rofy tinge remained in the oxalate. Nitrate precipitated by oxalate of ammonia.

Exp. 6. As the liquor from which the oxalate of cerium was precipitated contained an excefs of acid, it might be prefumed to hold in folution moft of the oxalate of lime formed at the fame time, if the cerite contained any. Accordingly it was mixed with the water that had wafhed the precipitate and concentrated by evaporation, when on the addition of ammonia a fmall quantity of oxalate of lime was thrown down. Oxalate of lime feparated.

Exp. 7. As notwithftanding fome oxalate of lime might have been precipitated with the oxalate of cerium, a portion of the Reft of the lime feparated.

red

red oxide of cerium arifing from the decompofition of the oxalate by calcination was diffolved in muriatic acid. A brifk effervefcence inftantly took place, with the evolution of oxigenated muriatic gas, which continued till the whole was diffolved, and differed in no refpect from that prepared with oxide of manganefe.

Muriatic folution of cerium rendered folid by ammonia.

The folution of cerium in muriatic acid was clear, and had only a light rofy tinge. To feparate it from the lime, if there were any, ammonia was added, when the folution, having been diluted with but a fmall quantity of water, congealed into a femitranfparent gelatinous mafs, which it was neceffary to agitate with a great deal of water, before it could be gotten out of the bottle.

The precipitate being wafhed and calcined was very compact, and had a brilliant fracture.

The liquor thus decompofed by ammonia contained lime, as appeared on precipitating it with oxalate of ammonia.

Oxalate of cerium.

At the inftant when the oxalate of cerium is precipitated by ammonia it is white and femitranfparent; but by agitation in the air and deficcation it affumes a yellowifh colour, and becomes opake. A remarkable circumftance is, that, if it be boiled with ammonia or potafh before it is dry, it becomes again perfectly white and opake. This is not owing to any combination of the alcalies with the cerium, for when it has been well wafhed, no trace of them can be difcovered by the moft careful analyfis.

Does not combine with alkalies.

Component parts of cerite.

The refiduum left untouched by the acids was afterwards examined; when it appeared, that the pureft ore of cerium * from Baftnas contained in 100 parts,

Oxide of cerium - -	63
Silex - - -	17·5
Oxide of iron - -	2
Lime - - -	3 or 4
Water - -	12
	98·5 †

Cerium,

* Mr. Vauquelin analyfed other fpecimens, which were mixed with green actinote and cupreous pyrites; but as nothing particular occurred in thefe analyfes, it is unneceffary to enter into them.

† Meffrs. H. and B. fay: filex 23 parts, carbonate of lime 5·5, oxide of iron 22, and of oxide of cerium after calcination more than 50.

Cerium, like several other metals, appears susceptible of two very distinct degrees of oxigenation: the oxide which contains least oxigen is white; that which is saturated with it is of a fallow red. Though they differ considerably in certain respects, their quantities of oxigen are not very dissimilar, whence they are readily and easily commutable into each other. Cerium has two oxides.

The white oxide exposed to the blowpipe soon becomes red, but does not melt, or even agglutinate. With a large proportion of borax it melts into a transparent yellow globule *: with less the globule becomes opake on cooling. On heating gently a transparent compound of borax and oxide of cerium it becomes milky like a tin enamel. Exposed to the blowpipe with borax.

The white oxide of cerium becomes yellowish in the open air, but never so red as by calcination, because it readily combines with carbonic acid, which opposes its union with oxigen to the point of saturation, and because it always retains a portion of water, which diminishes its colour. Takes oxigen and carbonic acid from the air.

Caustic potash by the assistance of heat deprives the red oxide of part of its oxigen, and renders it white. This being dried, however, and urged to the state of fusion, becomes red again. Alcalies have no other action on it. Alcalies do not act on it.

Sulphuric acid dissolves the red oxide with great difficulty. Equal parts of it and of sulphuric acid diluted with four times its weight of water combine readily when heated: the whole mass assuming a crystalline form and brilliant aspect. On adding fresh acid, and heating them together a long time, a complete solution takes place. This solution being evaporated by a gentle heat crystallizes in very small needles, some of which are orange †, others of a lemon colour. If evaporated quickly, nothing but a yellow powder is obtained. Sulphuric acid with the red oxide. Two sulphates.

50. The increase of weight they ascribe to oxigen absorbed by the iron and the cerium.

* First blood-red, then, as the heat decreases, green, yellowish, and finally colourless. If it be kept in the middle of the flame it continues as clear and colourless as glass. These phenomena are more evident, if a phosphoric salt be employed. If two colourless transparent globules, one formed with borax the other with a phosphoric salt, be fused together, a transparent compound is produced, which on cooling becomes opake, and of a pearl colour. Messrs. H. and B. With a phosphoric salt.

† These Messrs. H. and B. consider as an acidulous sulphate of cerium at a maximum of oxidation.

The

The sulphate of cerium is soluble in water only by means of an excess of acid. Its taste is saccharine and acid.

With the white oxide.

Sulphuric acid easily combines with the white oxide, particularly in the state of carbonate. The solution is colourless, or with a slight rosy tinge; of a saccharine taste without any perceptible acidity; and readily affords white crystals.

Nitric acid with the red oxide.

Nitric acid does not readily dissolve the red oxide unless assisted by heat. If the acid be superabundant, the solution yields white deliquescent crystals: if not, no crystals are formed, but a yellowish salt is formed by desiccation, of which alcohol at 38° will dissolve half its weight. The nitrate of cerium is decomposable by heat, and leaves a brick-coloured oxide.

With the white.

The white oxide unites more readily with nitric acid, but this salt is not more easily crystallizable. Its taste is at first pungent, afterward very sugary.

Muriatic acid.

Muriatic acid dissolves the red oxide with effervescence. The solution crystallizes confusedly. The salt is deliquescent, soluble in an equal weight of cold water, and in three or four times its weight in alcohol. The flame of this solution acquires no colour from the salt, but if agitated, white, red, and purple points appear in it *.

Oxigenated muriatic acid.

Oxigenated muriatic acid has no action on the red oxide, but dissolves the white, without yielding to it any of its oxigen.

Carbonic acid.

The oxide of cerium unites easily with carbonic acid. The most simple and ready method of forming this compound is to decompose a solution of the nitrate or muriate of the white oxide by saturated carbonate of potash, when a very white precipitate will be formed with effervescence, which is very light, and on drying assumes a shining silvery appearance.

Hidro-sulphures separate iron from cerium.

Sulphurated hidrogen does not combine with cerium: but hidrosulphures may be employed successively to separate any iron that may be mixed with it; for, when this is the case, the first portions of hidrosulphure will throw down from the solution of cerium a greenish precipitate till no more iron remains.

Tartarous acid.

The white oxide will unite directly with tartarous acid, but requires an excess of the acid to render it soluble †.

Mr.

* When this solution is concentrated it burns with a yellow sparkling flame. Messrs. H. and B.

† Messrs. H. and B. have observed, as well as Mr. V. that, if the

Mr. Vauquelin made feveral unfuccefsful attempts to reduce this metal; at firft he ufed the oxalate made into a pafte with fat oil. However, having mixed tartrite of cerium with a very fmall quantity of oil and lamp-black, he put it into a crucible of charcoal bedded in fand in an earthen crucible, and heated it for an hour and half in a forge furnace. A metallic globule fcarcely as large as a pin's head was now left in the coal, but no other trace of cerium could he difcovered, though the fand was examined with the utmoft care. Reduction of the metal.

None of the fimple acids acted on this globule, but it diffolved, though with extreme difficulty, in aqua regia, after being triturated. The folution was reddifh, and exhibited unequivocal marks of iron: but it likewife gave evident figns of the exiftence of cerium, both by its faccharine tafte, and by the white precipitates which tartrite of potafh and oxalate of ammonia threw down. The metallic globule too was harder, much more fragile, more fcaly in its fracture, and more white than pure caft iron. The globule examined.

As from thefe experiments cerium appears to be volatile, a fimilar mixture with the addition of borax was heated in a porcelain retort, to the neck of which a porcelain tube was adapted. Whether from the infufficiency of the heat however, or from the metal being volatilized without adhering to the neck of the retort, no trace of fublimate was found. But feveral very fmall metallic globules remained in the retort, adhering to a black varnifh formed by the borax. There were fome of thefe globules about the upper part of the veffel, to which apparently they had been fublimed by the force of the fire; but all thefe globules together would not have amounted to a fiftieth part of the cerium employed. Volatile, but attempt to fublime it fruitlefs.

the falts of cerium, decompofed by tartrite of potafh ftill contain traces of iron, the iron remains diffolved in the liquor, particularly if a flight excefs of tartrite be employed. Accordingly they have propofed this method as the beft and fimpleft for freeing the cerium from iron. The procefs they recommend for obtaining pure oxide of cerium is, to diffolve in nitro-muriatic acid any quantity of cerite, carefully felected and thoroughly calcined. To filter the folution, neutralize it by cauftic potafh, and then precipitate by tartrite and potafh. The precipitate well wafhed, and afterward calcined, is pure oxide of cerium. Simple and ready method of freeing cerium from iron, and obtaining the oxide pure.

Abftract

IX.

Abſtract of a Memoir, entitled Conſiderations on Colours, and ſeveral of their ſingular Appearances; read at the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute, March, 1805, *by* C. A. Prieur *.

Object of the memoir.

OUR author here endeavours to account for ſeveral phenomena, which appear to him never yet to have been properly explained: or rather it is his object to exhibit a general theory, by means of which all caſes of coloured appearances, even the moſt extraordinary, may be referred to certain principles.

Begins with the colours reſulting from a mixture of rays.

He ſets out from the known opinions concerning the various ſpecies of luminous rays, the colours reſulting from a mixture of ſeveral of theſe rays taken at different parts of the ſolar ſpectrum, and among others the very remarkable caſe, where the rays are ſo choſen, that their union produces on the organ of ſight the ſenſation of whiteneſs, even if two ſorts of rays only be employed.

For which we are indebted to Newton.

For theſe ideas we are indebted to the diſcoveries of the immortal Newton, and they flow immediately from the method he has propoſed for determining what colour would be obtained from a mixture of certain quantities of other given colours.

Preliminary requiſites.

If we would thoroughly comprehend what paſſes in the ſeeing of colours, it is indiſpenſable in the firſt place to obtain a familiar acquaintance with the ſhades compoſed of ſeveral ſimple rays; to acquire preciſe ideas of *black* and of *white*, and of the complication theſe introduce into coloured appearances; and more eſpecially to underſtand the relation of colours, which, taken two and two in a certain order, are capable of forming by their union white, or if you pleaſe any other compound tint.

Complementary colours.

Two colours having this kind of relation to each other are reciprocally termed *complementary colours*: one of theſe being given, the other may be determined with more or leſs preciſion by various modes of experiment, calculation, or ſimple reaſoning; and the conſideration of them applies very uſefully to a great number of caſes, as will be ſeen farther on.

* Tranſlated from the *Annales de Chimie*, Vol. LIV. p. 5, April, 1805.

3 We

We here pass over many particulars, which persons versed in the science of optics, or habituated to the practical application of colours, will easily supply. Besides, the subsequent part of the memoir, of which we have undertaken to give an account, will furnish an opportunity of repeating what is most necessary for understanding these subjects.

After these preliminaries the author proceeds to observations on *contrasts*. He employs this word to characterize the effect of the simultaneous vision of two substances differently coloured, when brought near together under certain circumstances. Contrast then is here a comparison, from which results the sentiment of a certain difference, great or small. It is pretty generally known, and painters in particular are well aware, that a coloured substance occupying a space of little extent, and placed near or surrounded by a given colour, has not the same appearance as in the neighbourhood of another colour: but whence arises this difference? — Contrasts.

Before we attempt to answer this question, let us make an essential distinction. The colours in question must be either homogeneal, that is formed of one sort of rays only; or compound, that is formed of a mixture of different rays.

In the first case, it must be confessed, we are ignorant, whether the approximation of different simple colours would produce any alteration in their respective appearance. As we seldom have an opportunity of seeing exhibitions of colour of this kind, and it is not easy to arrange such at will, no experiments have yet been made on their contrasts. The subject, however, is well worth studying. — Contrasts of simple colours not yet examined.

As to compound colours, and such are almost all those of natural or artificial substances, as our author shews in the course of his paper, the new colours exhibited by contrast are always conformable to the tint that would be obtained by abstracting from the colour proper to one of the substances the rays analogous to the colour of the other. — Its effect produced by abstracting from a colour the rays analogous to that contrasted with it.

Thus if we place on red paper a slip painted orange-colour, the latter will appear nearly yellow: on the contrary, the same strip placed on yellow paper will appear nearly red. If we place it on violet paper it will resume a yellowish tint, but different from the former; and lastly, on green paper it will appear red, but in a different degree. — Orange on red appears yellow; on yellow, red; on violet, yellowish; on green, another red;

 The

because orange consists of all rays but blue.

The explanation of these instances by the rule proposed is easy, if we suppose the orange-colour of the little strip to be compounded of all the rays except blue, which is commonly the case.

A multitude of combinations of colours thus placed upon one another, bring out the colour of contrast indicated by the rule above laid down; but there are several circumstances that render the effect more striking, or modify the result.

Contrast modified by circumstances.

Degree of light.

Sometimes it depends on the degree of light by which the colours are observed. They may be illumined uniformly, or some more than others. The quantity of light entering simultaneously into the eye from the whole field of view, has likewise its influence. If the colours form several surrounding borders to each other, as a series of circles decreasing in size and placed one upon another would do, they will act reciprocally on each other. At every junction there will be on each side a border coloured by the contrast of the adjacent tint. These borders will be of greater or less extent in proportion to the brightness of the colour. The effect of a single one may be sufficient to deaden or annihilate all the rest.

Many contrasts at once.

Effect increased by slight fatigue of the eye.

The colours of contrast will appear likewise with greater vividness after having observed them a few moments, or if the coloured substances be shaken a little, so that they may pass slowly over the retina. It seems as if a certain fatigue of the eye, either instantaneously with regard to the intensity of the light, or more slowly by a prolonged vision, concurred to produce the appearances in question. But an excessive fatigue of the organ would produce a degeneration of the colours belonging to another mode.

But not by excessive.

Colour on the retina after exposure to strong light, not from contrast.

We ought not therefore to refer to contrast those impressions mentioned by Æpinus, which are propagated in the eye with a certain duration, and a particular period of tints, when we have looked stedfastly on a very brilliant light, as that of the sun.

Buffon's accidental colours are

But the colours termed by Buffon *accidental*, on which Scherfer has written an interesting essay, belong to the class of contrasts, or at least constantly observe the same law.

coloured shadows of the same nature;

Coloured shadows are another phenomenon of the same kind. Count Rumford has established this fact beyond question in two

two essays, where he has treated the subject in a very pleasing manner.*

Mr. Prieur thinks, that those appearances of the solar light received through a hole in a coloured curtain, which General Meusnier had remarked on account of their singularity, are also to be ascribed to contrast. With this too he assimilates several cases of colours displayed by opals, or, to speak more generally, by bodies including perceptible opake parts disseminated through a transparent substance. In the same way he explains the colours under which the grayish dust collected by age on papers, or on coloured stuffs, appears; and he draws the same inferences with respect to the blueish appearance of the veins of the human body.

also light through hole in coloured curtain; opals; old dust on paper and cloth, and blueness of the veins.

He likewise proposes a new method of rendering the colours of contrast very sensible, more so than even by the known process of accidental colours, and nevertheless without occasioning any extraordinary fatigue of the eye. This last circumstance is of no small consequence, for every one must be aware, that so delicate an organ cannot be strained by over exertion without danger.

This method consists, the observer being in a room with a good light, in placing against the window the coloured papers, on which he means to observe the contrasts in the manner above mentioned. The coloured paper serving as the ground will then possess a degree of semitransparency, while the little slip of a different colour placed upon it is more opake, and in the shade, on account of the double thickness of paper: thus the colour produced by the contrast is rendered much more striking.

Method of rendering contrasts very sensible.

From this arrangement too results the singularly striking effect of contact of a little slip of white paper applied successively on paper, glass, and cloth of a given colour. When the transparent body is red, the opake white appears blueish green; if the ground be orange, it is decidedly blue; on a yellow ground, a kind of violet; on a crimson ground, green, &c.; always corresponding exactly to the *complementary colour*.

Slip of white paper on coloured paper, glass, &c.

On this it must be observed, that, according to the rule already mentioned, if we abstract from white, which is a com-

Explanation.

* See his Philosophical Works, Vol. I. p. 319, and following.

pound of all the coloured rays, the red rays for example, the remaining pencil ought to appear a very pale blueiſh green: but, as in the experiment above the little white ſlip is in the ſhade, the black hence ariſing may be of a proper degree to deſtroy the effect of the white, and then the blueiſh green appears of a lively tint. The ſame reaſoning is applicable to the caſe of all the other colours.

Reflected light muſt be avoided.

To obtain the full effect in repeating theſe experiments, we muſt take care, while procuring a favourable light, to guard againſt the reflection of adjacent bodies, and againſt double coloured fringes. Thus when the bright light tranſmitted through the window ſurrounds the tranſparent paper, it may very ſenſibly augment the brightneſs of the colour of contraſt, or injure it by introducing another tint, according to the colour of the body under obſervation. We have it always in our power, however, to get rid of this ſupercompoſition, by taking a piece of black cloth or paſteboard to maſk the object thus incommoded, or by looking through a blackened tube ſo as to confine the field of viſion to the neceſſary extent.

How.

Uſeful in the arts.

This knowledge of contraſt may be uſefully applied to thoſe arts, which are employed on the ſubject of colours. The painter is aware, that it is not a matter of indifference what colour is placed near another: but when he is acquainted with the law, to which their action on each other is ſubjected, he will know better what to avoid, and how to diſpoſe his tints, ſo as to heighten the brilliancy of that which he wiſhes to bring forward. Contraſting them together in ſucceſſion likewiſe affords us valuable indications of their nature and compoſition. This the author himſelf has put in practice with advantage in his manufactory of colours and paper-hangings.

White appearance of a coloured body through glaſs of the ſame hue.

Theſe conſiderations on contraſts led him to the examination of a very ſingular caſe, which Mr. Monge has mentioned and treated with his uſual ſagacity *. This caſe is the white appearance, which a coloured body ſometimes exhibits when viewed through a glaſs of the ſame hue. There remained ſome uncertainty reſpecting the circumſtances actually neceſſary for producing this effect: theſe our author determines by particular experiments, and he enumerates thoſe which have a favourable influence or the contrary. His concluſion is, that,

* *Annales de Chimie*, Vol. III.

when

when we have the perception of whitenefs in thefe cafes, it is owing folely to the action of contrafts, by which the impreffion of the colour is deadened or annihilated; while that of a certain degree of brightnefs ftill fubfifts, and is noticed from the oppofition of a greater degree of obfcurity. This manner of confidering the fubject leads to a new definition of whitenefs which has certainly nothing in it inconfiftent: *white is with respect to us the fenfation of light, when no particular colour predominates in it, or is perceived in it.* New definition of whitenefs.

In the fubfequent part of his memoir our author particularly confiders the colouring of different opake and tranfparent bodies; that is to fay, he inquires what are the luminous rays which a given coloured body is really capable of reflecting or tranfmitting. Further fubject of inquiry.

His method of making his experiments is fimple. If the fubftance be opake, he places it on a piece of black cloth, and obferves it with the prifm. If it cannot be cut fo as to reduce it to a rectangular figure, he covers it with a piece of blackened pafteboard, in which there is a rectangular aperture. Under thefe circumftances the coloured fringes difplayed on two oppofite fides indicate the kind of rays reflected, and confequently thofe abforbed when we know the nature of the illuminating pencil. On which we have farther to remark, that, as the fringes are themfelves compound tints, the fimple tints that compofe them muft be difcriminated. Their infpection fuffices an experienced perfon for this; but the habit is to be acquired, and its place fupplied, by taking for a guide papers reprefenting each fpecies of rays, placing them in their order one upon another, and drawing them back in gradation conformably to their difference of refrangibility: or we may ufe a table conftructed after Newton's method for determining the compound tints of feveral elementary colours. Method of making his experiments. Compound tints to be difcriminated. How.

If the body to be examined be tranfparent, the aperture in the pafteboard juft mentioned will be well adapted to cover it when placed againft the light, fo that the prifm may exhibit fringes on it. Or, if the obferver place himfelf in the dark, a light, as that of a candle, will exhibit through the tranfparent fubftance, by the affiftance of the prifm, a feries of coloured images correfponding to the rays tranfmitted. Method of examining tranfparent bodies.

Making his experiments in this manner, our author difcovered that feveral opake fubftances which happened to be at hand, Colours of opake bodies owing to abforption.

hand, of various natures and of all colours, whether yellow, orange, red, or green, blue and violet, owed their coloured appearance to the following laws: 1ſt, each of the bodies always abſorbed the rays that were complementary to the predominant colour: 2dly, in ſome the abſorption included, beſide the complementary ſpecies, others collateral to this ſpecies, and more or leſs numerous: 3dly, the deeper a colour is, the fewer ſpecies of rays it reflects.

Laws.

Relates to chemical compounds, not mechanical mixtures.

It is to be underſtood that mixed colours are not here ſpoken of, but only thoſe that form a homogeneal compound, or a true *combination*, in the ſenſe in which chemiſts uſe this word. Nor muſt the colour reflected from the interior of the molecules, ſuſceptible of light or deep tints, be confounded with the light reflected from the anterior ſurface of bodies: and though this mixes more or leſs with the proper colour, it is eaſy to diminiſh its effects, and diſcriminate them in the experiments.

Predominant colour.

Another remark proper to be made, is, that the expreſſion *predominant colour* muſt not be ſuppoſed to imply, that the rays of this colour are more abundant than the reſt, which would be a miſtake. Several ſpecies of rays may exiſt together in the pencil producing the colour, without any one ſpecies being for this reaſon more abundant. Strictly ſpeaking, all the elements of the pencil are diſſimilar; and conſequently no one exiſts in it in greater quantity. But the general tone of colour remains analogous to that of the rays ſtyled predominant; for which reaſon it is well to retain the term, provided it be not taken in an exaggerated ſenſe.

Tranſparent bodies follow the ſame law of abſorption.

The author has likewiſe obſerved tranſparent bodies, ſuch as coloured glaſs of different ſorts, and liquors contained in a bottle with two broad parallel ſides. For theſe he found the ſame law of abſorption as for opake bodies, but ſtill more marked, and free from all doubt.

Its modifications.

This law is conſtant and regular. It depends on the nature of the body receiving the light, its denſity, and its thickneſs. It is likewiſe modified by the intenſity of the light of the illuminating body, and the kind of rays that compoſe this light.

Progreſs of the abſorption of rays.

The abſorption always commences with the rays moſt oppoſite to the predominant colour of the body illumined. It goes on to thoſe which come next in the ſpectrum; and thus proceeds regularly from one order of rays to the next in ſucceſſion,

cession, never by fits, till it reaches the last. In consequence the body grows darker and darker, and always finishes with becoming black. Sometimes it extends only on one side from the rays first absorbed; at other times on both sides at once, and either with equal pace, or more rapidly on one side than on the other.

Change of circumstances varies the results.

If we vary each particular that affects the experiment separately, we shall have a distinct progression of results. That depending on the density of the substance is not always similar to that arising from change of thickness. In receiving light of different kinds too on the same substance, the progress of absorption is differently modified, and consequently the colours changed.

Our author adduces instances of all these cases. He takes them from the numerous experiments he has made with coloured glass, acid, or alcaline solutions of metals, and fluids tinged by the infusion or decoction of vegetable substances. These exhibit curious particularities, but we shall not here relate them, both for the sake of brevity, and because it is easy for any person to observe them, when once the track is pointed out.

May lead to the cause of permanent colours.

From all these observations taken together, many very important consequences respecting the reciprocal action of bodies and light on each other are drawn; and perhaps at some future period they will tend to elucidate the grand question concerning the cause to which their permanent colours are to be ascribed.

Colour of bodies at different degrees of incandescence.

Lamps seen through mist. Sun through smoaked glass.

After these hints, the author dedicates a concluding paragraph to the examination of several phenomena of different kinds. He points out the modifications that coals heated to different degrees of incandescence undergo in their colours. His remarks apply to other substances likewise, as iron in the state of ignition, a long row of lamps with reverberators seen through a fog, or a white light seen through a glass blackened by progressive applications of smoke. In all these cases the colours necessarily pass through a series of tints from white to yellow, orange, and red of a deeper and deeper shade, the reason of which he gives.

Colours of metallic oxides proportionate to the oxigen.

Metallic oxides too have a gradation of tints, according to their proportion of oxigen. A certain continued change in

vegetation

Of flowers, &c. vegetation produces the same effect on some parts of plants. The arts and chemical processes exhibit the same in a multitude of circumstances.

Use to the manufacturer. Hence the manufacturer may derive with advantage indications either of the progress of combinations, or of the proper instant for executing certain parts of his operations.

Coloured clouds. Our author next enters more particularly into the appearance of coloured clouds, particularly those we see about the rising and setting of the sun. This phenomenon so generally known, had hitherto remained without explanation, though this had been attempted by natural philosophers of the first rank.

Owing to absorption of light, not to refraction. It is not owing to the refraction of the solar rays, but to the successive absorption of them, when they strike on the inferior parts of the atmosphere, which are more loaded with vapour.

This absorption follows laws analogous to those already mentioned. The quantity of vapours, and even their nature not being the same every day, produce corresponding differences in their effects.

Order of absorption. Commonly the first rays attacked by these vapours are the blue adjacent to the violet. Soon after they attack the contiguous rays, gaining with more rapidity the blue properly so called; then the green, the yellow, and thus proceeding to the red. Hence the yellowish, orange, and red colours exhibited by the clouds. This period of tints, the evening for example, displays itself gradually as the sun approaches the horizon. The same hues tinge terrestrial objects, the part of the air nearest the sun, and this luminary itself. Accordingly when we can receive its rays on a prism, we perceive that the rays actually absorbed correspond to the general tint of the moment.

Sun-set.

From the successive increase of the vapours traversed by the light in thickness and density, it follows likewise, that at the same instant clouds differently placed must be clothed in different hues. The highest may be white, while others not so high are yellow, and others still lower proportionately more red. At equal elevations those furthest from the point where the sun sets will incline to red, and those nearest it to yellow.

Blue and green shadows, owing to contrast. We may then see blue or green shadows on bodies naturally white, as Buffon and other philosophers have observed. These, as has been said, are nothing more than the effect of contrast between

between the actual colour of the part enlightened, and that of the part in shade.

Contrasts affect the colour of the clouds.

Contrasts may likewise render the colour of the clouds complicated, as for instance, when a great portion of the sky displays its blue tint. There are some clouds, the colour of which arises solely from this cause; and such may be seen at times in the middle of the day, when we have a lofty mountain at our back, or are in any other situation where the eye is defended from the too powerful action of the solar light, either direct or reflected; but in this case the clouds have only a yellowish tinge, precisely the complementary colour of sky blue.

The Moon.

Sometimes we see the moon of a similar colour, when it is very high, a little before or after the sun passes the the horizon; farther it appears thus, or even completely white, when clouds variously coloured by the vapors of sun-set or sun-rise exist in the air at the same time. From this concurrence of circumstances we have a new proof of the difference of causes to which these colours are owing.

Lastly let us remark, that from the irregularity of the earth's surface, and of the state of the atmosphere, the phenomena are liable to be concealed or subjected to various interruptions. In our climate the colouring of the clouds seldom reaches its last stage. On some evenings however, when the sky is very clear toward the part where the Sun sets, while light clouds float very high over our heads, we shall see these at a subsequent period appearing of a very light red, heightened by the diminution of light on the earth, soon after obscured, and at length becoming extinct in shade.

Red clouds over head at sun-set.

Conclusion.

The theory of colours imperfect.

Notwithstanding the many beautiful discoveries already made respecting light, the theory of the production of colours has not yet attained a degree of generalization that renders it applicable to all cases, or that simplicity of principles to which we are almost always led when we have discovered the real laws of nature. Many phenomena have eluded explanation, and that given of several requires correction. Our author has proposed to establish alterations in the theory, the necessity of which he points out. He supports his principles partly by the doctrine and facts generally admitted; partly

by

by others less commonly known, though of ancient date; and lastly by observations of his own. He is far from flattering himself, however, that a sketch like the present exhibits the matter in a suitable light; and was soon aware that a subject so extensive and so complicated required maturer labours.

The author intends to pursue the subject.

To fill up many gaps, unfold various points, and correct and extend others by farther researches, new experiments, and profound reflections, is an ample field of improvement; and this he will attempt, if his powers and his leisure will permit.

It would likewise be useful, as well as just, to give at the same time an abstract of what we owe to the genius of the great Newton, who opened the career in such an admirable manner, and to those philosophers who have discovered new facts, or removed difficulties. Greater precision also should be introduced into the language which we employ respecting colours, proportionate to the increase of our knowledge, and the actual state of the arts and sciences. Lastly, in a subject like the present, it would not be too much to add the resources of algebra and geometry to the treasures of experiment, and if possible to the advantages of a better method.

X.

Report made by the Physical and Mathematical Class of the Institute in Answer to the Question, whether those Manufactories, from which a disagreeable Smell arises, may prove injurious to Health. Read in the Sitting of January, 1805, *by Messrs.* GUYTON-MORVEAU and CHAPTAL.*

THE minister of the home-department has consulted the class on a question, the solution of which is of essential import to our manufacturers.

Question.

The object is to determine, whether the vicinity of certain manufactories can be injurious to health.

Its importance.

The solution of this problem must appear of the more consequence, as, from the confidence which the decisions of the Institute naturally merit, it may hereafter form the basis of

* Translated from the *Annales de Chimie*, vol. LIV. p. 86, for April, 1805.

decisions

decifions in a court of juftice, when fentence is to be pronounced between the fate of a manufactory and the health of our fellow-citizens.

The folution is fo much the more important, it is become fo much the more neceffary, as the fate of the moft ufeful eftablifhments, I will fay more, the exiftence of many arts, has depended hitherto on fimple regulations of police; and that fome, driven to a diftance from materials, from workmen, or from confumers, by prejudice, ignorance, or jealoufy, continue to maintain a difadvantageous ftruggle againft innumerable obftacles, by which their growth is oppofed.

Thus we have feen manufactories of acids, of fal ammoniac, of Pruffian blue, of beer, and of leather, fucceffively banifhed from cities; and we daily fee appeals to authority againft thefe eftablifhments made by troublefome neighbours or jealous rivals. Manufactories objected to.

As long as the fate of thefe manufactories is infecure, as long as an arbitrary legiflation poffeffes a right to interrupt, fufpend, or fetter the hands of a manufacture; in a word, as long as a fimple magiftrate of police has at his nod the fortune or ruin of a manufacturer, how can we conceive, that he will be fo imprudent as to engage in undertakings of fuch a nature? How could it be expected, that manufacturing induftry fhould eftablifh itfelf on fuch a frail bafis? This ftate of uncertainty, this continual conteft between the manufacturer and his neighbours, this perpetual doubt refpecting the fate of an eftablifhment, paralyfe and confine the efforts of the manufacturer, and gradually extinguifh both his courage and his powers. Difadvantage of having no fixed rules.

It is an object of primary neceffity therefore to the profperity of the arts, that lines fhould be drawn, fo as no longer to leave any thing at the arbitrary will of the magiftrate; to point out to the manufacturer the circle in which he may exert his induftry with freedom and fecurity, and to affure the neighbouring proprietor, that he has nothing to fear for his health, or for the produce of his fields.

To arrive at the folution of this important problem, it appeared to us indifpenfable, that we fhould take a view of each of the arts, againft which the moft clamour has been raifed.

With this view we fhall divide them into two claffes. The firft will comprife all thofe, the proceffes of which allow aeriform emanations to efcape from them into the atmofphere, either in Claffification of objectionable trades.

in consequence of putrefaction or fermentation, which may be deemed nuisances from their smell, or dangerous from their effects.

The second class will include all those, in which the artist, operating by the aid of fire, developes and evolves in air or vapour various principles, which are more or less disagreeable to respire, and reputed more or less injurious to health.

1st class. In the first class we may advert to the steeping of flax and hemp, the making of catgut, slaughter-houses, starch-manufactories, tanneries, breweries, &c.

2d class. In the second, the distillation of acids, of spirits, and of animal substances; gilding on metals, preparations of lead, copper, and mercury, &c.

1st class injurious to health. The arts of the first class, considered in relation to the health of the public, merit particular attention, because the emanations that proceed from fermentation or putrefaction are really injurious to health in some cases, and under certain circumstances: the steeping of flax and hemp for instance, which is performed in ponds or still waters, infects the air and kills fishes; and the diseases to which it gives rise are all known and described: Accordingly wise regulations have almost every where enjoined, that this operation should be carried on without the precincts of towns, at a certain distance from every dwelling, and in waters, the fish of which constitute no resource for the public. These regulations unquestionably ought to be continued; but as the execution of them is attended with some inconvenience, it is to be wished, that the process of Mr. Brale, the superiority of which has been confirmed by Messrs. Mongez, Berthollet, Tessier, and Molard, should soon become known and adopted.

Steeping hemp.

Brale's method recommended.

Beer, vegetable colours, starch, paper, &c. Other operations on vegetables, or certain products of vegetation, to obtain fermented liquors, as in breweries; to extract colours, as in the manufactures of litmus, archil, and indigo; or to divest them of some of their principles, as in manufactories of starch, paper, &c. do not appear to us of such a nature as to be capable of exciting any disquietude in the mind of the magistrate. At all events the emanations arising from these substances in a state of fermentation can prove dangerous only near the vessels and apparatus in which they are confined, ceasing to be so the moment they are mingled with the open air; so that a little prudence only is required, to avoid all danger

danger from them. Besides, the danger affects only the manufacturers themselves, and by no means the inhabitants of the neighbouring houses, so that a regulation enjoining these manufactories to be removed out of towns, and to a distance from any dwelling-house, would be an act of authority both unjust, vexatious, and injurious to the progress of manufactures, and in no respect a remedy for the evils attending the operation.

Some preparations extracted from animal substances require the putrefaction of these substances, as in the fabrication of catgut; but it is more frequently the case, that animal substances employed in manufactures are liable to putrefaction from being kept too long, or exposed to too great warmth, as we particularly find in dyeing cotton red, a process in which a large quantity of blood is employed. The miasmata exhaled by these putrid matters spread far round, and form a very disagreeable atmosphere for all the neighbourhood to breathe; it is the part of a good government, therefore, to cause these substances to be renewed so as to prevent putrefaction, and the manufactory to be kept so far clean, that no refuse of the animal substances employed shall be left to rot in them. Catgut. Dyeing cotton red.

In this last point of view slaughter-houses exhibit some inconveniencies; but they are not of sufficient importance to require them to be placed without the precincts of towns, and assembled together in one spot, as speculative men are daily proposing to government. A little attention on the part of the magistrate, to prevent butchers from throwing out the blood and refuse of the beasts they kill, would be sufficient to remedy completely every thing disgusting or unhealthy arising from slaughter-houses. Slaughter-houses.

The fabrication of *poudrette* (night-soil dried) begins to be established in all the large towns of France, and the operation by which excrementitious substances are reduced to this state, necessarily occasions a very disagreeable smell for a long time. Establishments of this kind therefore ought to be confined to airy places, remote from any habitation; not that we consider the aeriform exhalations from them as injurious to health; but no one can deny, that they are incommoding, noisome, disagreeable, and difficult to breathe, on all which accounts they ought to be removed to a distance from the dwellings of men. Poudrette.

There is a very important observation to be made on the spontaneous decomposition of animal substances, which is, that Animal putrefaction dangerous only in proportion to its humidity.

that the emanations from them appear to be so much the less dangerous, as the substances which undergo putrefaction are less humid: in the latter case, a considerable quantity of carbonate of ammonia is evolved, which imparts its predominant character to the other matters volatilised, and corrects the bad effects of such as are deleterious. Thus the decomposition of stercoraceous matters in the open air, and in places the situation and declivity of which allow the fluids to drain off, and that of the refuse of the cocoons of the silk-worm evolve a vast quantity of carbonate of ammonia, which corrects the virus of some other emanations; while the very same substances, decomposed in water or drenched with this fluid, exhale sweetish and nauseous miasmata, the respiration of which is very dangerous.

2d class. The numerous arts in which the manufacturer produces and diffuses in the air, in consequence of his processes and by the help of fire, vapours more or less disagreeable to breathe, constitute the second class of those we have to examine.

These, more interesting than the former, and much more intimately connected with the prosperity of our national industry, are still oftener the subject of complaints brought before the magistrate for decision, and on this account have appeared to us to require more particular attention.

We will begin our examination with the manufacture of acids.

Acids. The acids that may excite complaints of the neighbours against their preparation are the sulphuric, nitric, muriatic and acetous.

Sulphuric acid. The sulphuric acid is obtained by the combustion of a mixture of sulphur and nitre. It is very difficult in this process to prevent a more or less observable smell of sulphurous acid from being diffused around the apparatus, in which the combustion is performed; but in manufactories skilfully conducted this smell is scarcely perceptible within the building itself; is not dangerous to the workmen who respire it daily, and can give no reasonable foundation for complaint to the neighbours. When the art of making sulphuric acid was introduced into France, the public opinion was strongly expressed against the first establishments for the purpose; the smell of the match with which we kindle our fires contributed not a little to exaggerate the effect that must be produced by the rapid com-

 bustion

bustion of several hundred weight of brimstone; but men's fears on this head are now so much allayed, that we see several of these manufactories prosper in peace in the midst of our cities.

Aqua fortis and spirit of salt.

The distillation of aqua fortis and spirit of salt, in other words, of the nitric and muriatic acids, are not more dangerous than that of sulphuric acid. The whole of the process is performed in an apparatus of glass or earthen-ware, and it is unquestionably the great interest of the manufacturer to diminish the volatilization or loss of the acid as much as possible. Yet, let him pay whatever attention he will to this, the air breathed in the manufactory is always impregnated with the smell peculiar to each of these acids; but you may respire there freely and safely, the men who work in it daily are not at all incommoded by it, and the neighbours would be very much in the wrong to complain.

Vinegar.

Since the manufactories of white lead, of verdigris, and of sugar of lead have increased in France, the demand for vinegar has been enlarged.

Distilled vinegar.

When this acid is distilled, to fit it for some of the purposes for which it is used, it diffuses to a distance a very strong smell of vinegar, in which there is no danger; but when a solution of lead in this acid is evaporated, the vapours assume a sweetish character, and produce in those who respire them constantly all the effects peculiar to the emanations of lead itself. Happily these effects are confined to the people who work in the manufactory, and are unfelt by those who dwell in the vicinity.

Mineral preparations, and gilding.

The preparations of mercury and of lead, those of copper, antimony, and arsenic, and the processes of gilding on metals, are none of them without some danger to the persons who reside in those manufactories, and are concerned in the operations; but their effects are bounded by the walls within which they are carried on, and are dangerous only to the persons concerned in the manufactories. It is an object well worthy the attention of chemists, to investigate the means of preventing these injurious effects, and indeed many of the inconveniencies have already been prevented by the help of chimneys, which convey the vapours into the air out of the reach of respiration; and at present the whole attention of administration ought

ought to be confined to directing science toward the means of improvement of which these processes are susceptible with regard to health.

Prussian blue, and sal ammoniac.

The fabrication of Prussian blue, and the extraction of carbonate of ammonia by the distillation of animal substances in the new manufactories of sal ammoniac, produce a large quantity of fetid vapours or exhalations. These exhalations, it is true, are not injurious to health; but as it is not sufficient to constitute a good neighbour, not to be a dangerous one merely, but not even to be a disagreeable one, they who undertake such manufactures, when they have to seek a situation for them, should prefer one remote from any dwelling-house. But when such a manufactory is already established, we would be far from advising the magistrate to order its removal: it would be sufficient in such cases, to oblige the manufacturer to build very high chimneys, that the disagreeable vapours produced in these operations may be dissipated in the air. This is particularly practicable for the fabrication of Prussian blue, and by adopting it one of our number has continued to retain in the midst of Paris one of the most important manufactories of this kind we have, against which the neighbours had already leagued.

Few injurious to health.

In the report we lay before the class we have thought it our duty to attend only to the principal manufactories, against which violent clamours have been raised at divers times and places. It is easy to see, from what has been said, that there are but few the vicinity of which is injurious to health.

Caution to magistrates.

Hence we cannot too strongly exhort those magistrates who have the health and safety of the public committed to their charge, to disregard the unfounded complaints, which, too frequently brought against different establishments, daily threaten the prosperity of the honest manufacturer, check the progress of industry, and endanger the fate of art itself.

They should not listen too readily to complaints.

The magistrate ought to be on his guard against the proceedings of a restless or jealous neighbour; he should carefully distinguish what is only disagreeable or inconvenient from what is dangerous or injurious; he should recollect that the use of pit-coal was long prescribed, under the frivolous pretence that it was injurious to health; in short, he should be fully aware of this truth, that, by listening to complaints of this nature, not only would the establishment of several useful arts in France be

be prevented, but we ſhould inſenſibly drive out of our cities the farriers, carpenters, joiners, braſiers, coopers, founders, weavers, and all whoſe occupation is more or leſs diſagreeable to their neighbours. For certainly the employments juſt named are more unpleaſant to live near than the manufactories mentioned above, and the only advantage they enjoy is that of ancient practice. This right of toleration has been eſtabliſhed by time and neceſſity; let us not doubt therefore, but our manufactures, when grown older and better known, will peaceably enjoy the ſame advantage in ſociety; in the mean time we are of opinion, that the claſs ought to avail itſelf of this circumſtance, to put them in a particular manner under the protection of government, and declare publicly, that the manufactures of acids, ſal ammoniac, Pruſſian blue, ſugar of lead, white lead, ſtarch, beer, and leather, as well as ſlaughter-houſes, are not injurious to the health of the vicinity, when they are properly conducted.

Diſagreeable occupations ſanctioned by time.

Manufactures not injurious to health.

We cannot ſay as much for the ſteeping of hemp, making catgut, layſtalls, and in general eſtabliſhments where a large quantity of animal or vegetable matter is ſubjected to humid putrefaction. In all theſe caſes, beſide the diſagreeable ſmell they exhale, miaſmata, more or leſs deleterious, are evolved.

Injurious manufactures.

We muſt add, that, though the manufactories of which we have already ſpoken, and which we have conſidered as not injurious to the health of the neighbourhood, ought not to be removed, yet adminiſtration ſhould be requeſted to watch over them ſtrictly, and conſult with well-informed perſons for preſcribing to the conductors the moſt proper meaſures for preventing their ſmoke and ſmell from being diffuſed in the vicinity. This end may be attained by improving the proceſſes of the manufactures, raiſing the outer walls, ſo that the vapours may not be diffuſed among the neighbours; improving the management of the fires, which may be done to ſuch a point, that all the ſmoke ſhall be burnt in the fire-place, or depoſited in the tunnels of long chimneys; and maintaining the utmoſt cleanlineſs in the manufactories, ſo that nothing ſhall be left to putrify in them, and all the refuſe capable of fermentation be loſt in deep wells, and prevented from any way incommoding the neighbours.

Manufactures not injurious require ſome reſtrictions.

We ſhall obſerve too, that when new manufactories of Pruſſian blue, ſal ammóniac, leather, ſtarch, or any other article

New manufactories.

ticle by which vapours very inconvenient to the neighbours, or danger of fire or explosions are to be established, it would be wise, just, and prudent, to lay down as a principle, that they are not to be admitted into cities, or near dwellings, without special authority; and that, if persons neglect to comply with this indispensable condition, their manufactories may be ordered to be removed without any indemnification.

Summary. It follows from our report, 1st, that catgut manufactories, laystalls, steeping of hemp, and every establishment in which animal or vegetable matters are heaped together to putrify in large quantities, are injurious to health, and ought to be remote from towns and every dwelling house: 2dly, that manufactories where disagreeable smells are occasioned through the action of fire, as in the making of acids, Prussian blue, and sal ammoniac, are dangerous to the neighbours only from want of due precautions, and that the care of government should extend only to an active and enlightened superintendance, having for its objects the improvement of their processes and of the management of the fire, and the maintenance of cleanliness: 3dly, that it would be worthy a good and wise government, to make regulations prohibiting the future establishment of any manufacture, the vicinity of which is attended with any essential inconvenience or danger, in towns or near dwelling-houses, without special authority previously obtained. In this class may be comprised the manufactories of poudrette, leather, and starch; foundries, melting houses for tallow, slaughter houses, rag warehouses, manufactories of Prussian blue, varnish, glue and sal ammoniac, potteries, &c.

Such are the conclusions which we have the honour to lay before the class,* and addressed to government, with invitation to make it the base of its decisions.

* These conclusions were adopted by the Institute.

Facts

XI.

Facts relative to the Torpid State of the North American Alligator.
By BENJAMIN SMITH BARTON, M. D.*.

IT has not, I think, been remarked by the generality of the writers on natural hiftory, that the North American Alligator paffes during the prevalence of cold weather, into the torpid ftate. This however, is unqueftionably the cafe in fome parts of the continent.

Account by by Boffu.

Mr. Boffu, a French writer, after telling us that thefe animals are numerous in the Red River, one of the weftern branches of the Miffiffippi, fays, " they are torpid during the cold weather, and lie in the mud with their mouths open, into which the fifh enter as into a funnel, and neither advance nor go back. The Indians then get upon their backs, and kill them by ftriking their heads with hatchets, and this is a kind of diverfion for them †.

Dr. Fofter, the tranflator of the work, obferves in the preceding paffage, " that the circumftance of the alligator's being torpid during winter is quite new, and very remarkable for natural hiftory." It feems (he adds) almoft all the clafs of animals called *amphibia*, by Dr. Linnæus, when found in cold climates grow torpid during winter.

Another account by Mr. Graham.

In addition to the authority of Mr. Boffu, I may here mention the following fact, which was communicated to me about the year 1785, by a Mr. Graham, at that time a very intelligent ftudent of medicine in the Univerfity of Pennfylvania.

The alligator fwallows pine-knots previous to becoming torpid.

" The alligator having previoufly fwallowed a number of pine-knots, retires to his hole, where he remains in a torpid ftate, during the feverity of winter. If killed at this feafon, thefe knots are found highly polifhed by their trituration one against the other in the animal's ftomach, as I have more than once heard from men of undoubted veracity, who had

* From " the Philadelphia Medical and Phyfical Journal". Collected and arranged by Ben. Smith Barton, M. D. It is publifhed in half yearly Numbers, the firft of which appeared in November, 1804.

† Travels through that part of North America formerly called Louifiana. Englifh Tranflation, Vol. I. p. 367. London 1771.

been

been concerned in the fact. Indeed this is so notorious in those parts in which these creatures abound, that the digestion of the alligator's stomach is proverbial amongst the multitude, who deride its insipidity in the choice of such food, though, I presume, this it does instinctively, for some purpose unaccounted for by naturalists; and which, perhaps is beyond the limits of human ken."

The fact related by Mr. Graham, relates to the alligator of the Carolinas, in which parts of the United States this animal is very common. By another gentleman I have been informed, that the pine knots which the alligators swallow are generally such as are very abundant in turpentine. I have also been assured, by my friend Mr. William Bartram, that he has seen a brick-bat which was taken out of the stomach of an alligator, and that it was worn quite round.

Local situation of this animal, &c.

Mr. Lawson says, that the alligator is not seen to the North of North-Carolina. They are very common at Cape-Fear in latitude 34. One twelve feet in length has been seen at this place. On the Atlantic side of the United States I am not able to trace them farther than the "Alligator Dismal Swamp," which is between Edenton and Newbern in North-Carolina. The mouth of the Red River in latitude 31.

Within the tract of country just mentioned, the alligator obeying the impulse of the climate, passes into the torpid state. In North-Carolina this takes place about the middle of November, sooner or later, according to the state of the season. Whether the animal becomes torpid in more Southern parts of the Continent, I have not been able to learn. On the river St. John in East Florida, they have been seen awake even in the middle of winter, but it was remarked that they seemed dull and stupid. It has also been observed, that they are accustomed to frequent the warm springs which are so abundant in this part of the Continent; and that they are fond of lying in these springs. Perhaps the heat of these springs may be sufficient to prevent them from becoming torpid. But it must be observed, that a deficiency of heat is not the only cause of the torpid condition of animals.

Conjectures respecting their swallowing the knots of the pine.

It may not perhaps be an easy task to assign a satisfactory cause for the singular instinctive appetite, which leads the alligator, before going into the torpid state, to swallow pine-knots, and other somewhat similar substances. But I apprehend

prehend that these substances, when taken in by the animal, act in some measure by keeping up a certain degree of action in its stomach, and consequently in every part of the system, and thereby prevents the death of the animal, which might otherwise be destroyed by the long continued application of cold. Some facts mentioned by Dr. Pallas, though they respect a very different family of animals, render this conjecture not a little plausible *.

This subject is worthy of more attention. In particular, it will be well to enquire, whether the alligator does swallow pine-knots, stones, &c. in those parts of America in which it does not pass into the torpid state.

XII.

Observations and Experiments on the conducting Power of Fluids. By T. S. Traill, *M. D. From the Author.*

To Mr. NICHOLSON.

SIR, *Liverpool, Sept.* 10, 1805.

If you think the following observations and experiments worthy of a place in your excellent Journal, your inserting them will oblige,

Your obedient servant,

J. S. TRAILL, *M. D.*

Count Rumford was the first who maintained that fluids are absolute non-conductors of caloric. This conclusion he drew from the interesting fact he had discovered, of the extreme slowness with which ice melted when a stratum of cold water was interposed between it and the heated body. He imagines that it was always melted in such circumstances, either by *currents* produced, in some of them by changes in specific gravity, or by the transmission of caloric through the sides of the containing vessel. The experiments of this illustrious philosopher have roused the attention of the learned, and to the united labours of yourself, of Thomson, of Dalton, and

Doctrine of Count Rumford, that fluids are non-conductors of heat, controverted.

* Historia Glirium, &c.

and of Murray, we are indebted for an investigation of the Count's opinions, the result of which seems to be, that fluids are not absolute *non-conductors* of caloric.

Experiments of Dalton, Thomson, Nicholson, and Murray, considered by the Count.

The experiments of Dalton and Thomson have proved, that the appearances of currents, such as described by Rumford, may be often illusory; and from those of Nicholson, and from Murray's first experiments, we have strong reasons for supposing, that the temperature was affected by the conducting power of the fluids employed; but in my opinion the experiments of Murray with a cylinder of ice, are the most complete demonstration of this contested point. In a late paper, inserted in the Transactions of the Royal Society of London, Count Rumford endeavours to obviate these objections to his hypothesis, in his usual ingenious manner.

It is not apprehended that the experiment of Murray could be affected by currents:

Even admitting that in your experiments the caloric was transmitted solely by the containing vessel (an opinion by no means probable), and that *currents*, such as Rumford describes, have all the effect he attributes to them in certain cases; still the experiments of Mr. Murray appear to me incontrovertible. It was not, therefore, without surprise, that I observed him use the following argument to invalidate their results: "When that vessel is constructed of ice, the flowing "down of the water, resulting from the thawing of the ice, "will cause motions in the liquid, and consequently inaccu-"racies of still greater moment;" viz. than those produced by the conducting powers of the sides of the vessel. Now the melting of the ice could affect the thermometer only by being itself heated, and then trickling down the sides of the cylinder of ice. But I apprehend, the water resulting from the melting of the ice could not gain a higher temperature than 32° F. while it remained in contact with ice. If we mix even equal parts of water at 172° and ice, we do not find that the temperature of the mixture is above 32°. If such a large quantity of water cannot maintain its temperature in contact with ice, can we suppose that such a small quantity as was formed could rise to a higher while trickling down the sides of a thick cylinder of ice.

and certainly not that with mercury in a vessel of ice.

But even this explanation of the phenomenon advanced by Count Rumford, is entirely inapplicable to the experiment with mercury; for the drops of water formed could not possibly sink in a fluid so much more dense, nor throw it into currents which could reach the thermometer.

3 Besides

Besides those most ingenious experiments devised by Murray, we have other proofs of the conducting power of liquids in several well known facts.

Proofs that fluids are proper conductors.

1st. If the *non-conducting* power of liquids have any meaning, it must signify that their particles are incapable of communicating to each other the temperature they have acquired by physical contact with some other body, whose temperature was elevated. If this were true, how shall we account for the mean temperature produced by mixing equal quantities of hot and cold water? Rumford, if I recollect aright, has endeavoured to obviate this objection to his hypothesis by supposing, that it is only an intimate mixture of hot and cold particles which takes place in such cases. If this were true, we should expect, from the rapid motion he supposes the currents to have in liquids that are heated, that they would soon separate into warmer and colder strata, from the difference in their specific gravities: This however is not the case: The whole acquires a uniform temperature.

1. They take a common temperature on mixing.

2d. When mercury and water at different temperatures are mixed, an interchange of caloric takes place. From the very great difference of their specific gravities we cannot suppose that every particle of the one has been in contact with every particle of the other; yet they soon acquire a common temperature, which though not a mean, has always a constant relation to the temperature of the two fluids before mixture. Does not this indicate a considerable conducting power in those liquids? Indeed, I cannot conceive that any interchange of temperature could take place in such cases, if the particles of the liquids were incapable of communicating their caloric to the next particles.

2. More particularly water and mercury.

3rd. The beautiful experiment devised by Rumford, in which water, in a glass tube, was made to boil over a cake of ice, by the application of a heated body to the upper part of the containing tube, without, for a very long time, affecting the ice, is a sufficient proof of the slowness with which glass transmits caloric, and clearly indicates that the sides of the vessel in several of the experiments of the above-mentioned philosophers, could not be the sole conducting medium.

3. The vessel is too bad a conductor to account for the effects urged against the doctrine of C. R.

4th. The sixteenth experiment in Rumford's seventh essay, affords another argument against his opinion. He poured boiling water on a stratum of cold water, which rested on a cake

4. Hot water poured on cold does not raise its temperature by currents.

cake of ice in the bottom of a jar; he found that near the surface of the ice the temperature was 40°, at the distance of three inches it was 159°, but at the distance of seven inches it was only 160°. Had the cold water acquired its elevation of temperature by the currents produced, or by the sides of the vessel, we ought, I apprehend, to have found the temperature spreading more uniformly: but though the first four inches only differ by one degree, we find the next three differing by 119 degrees.

5. Heat applied to fluids downwards.

5th. If liquids were absolute non-conductors of caloric, it would necessarily follow, that when caloric was applied to the upper surface of different liquids, other circumstances remaining unchanged, and provided the liquid did not increase in specific gravity by cooling, *equal increments of temperature would take place in equal times.*

Apparatus for experiments of transmitting heat downwards through fluids.

From several experiments it is probable, that some liquids conduct caloric more rapidly than others. The following were undertaken with a view to ascertain more accurately this point: How far I have succeeded I leave you to judge:

A cylindrical vessel was turned out of wood, having its sides 0.5 inch thick; its height four inches, and its diameter two. It has a moveable wooden top or cover perforated with a hole in its centre a little more than an inch in diameter, into which an iron cylinder of one inch in diameter could be easily introduced. This cylinder is supported by a slight flanch or shoulder-piece, and can be taken up by means of a string attached to its top. When the iron bar is in its place, its flat lower extremity is 0.5 inch distant from the bulb of a delicate mercurial thermometer D E, which is fixed by wax, in a hole perforating the cylinder near its bottom. This thermometer, which was made by the late Ramsden, has a tube as fine as a human hair, and is bended to a right angle, so that its bulb and part of its stem lie in the axis of the wooden cylinder. This shape was preferred, because the stem could be little affected by the caloric transmitted by the sides of the vessel, till after the bulb was acted on by the caloric of the iron bar. A variety of experiments were performed with this apparatus in the following manner: The temperature of the room being steadily 67° F. during the trials, a kettle of water was kept boiling over the fire: Its temperature was between 211° and 212°, and into this the cylinder of iron was suffered to remain,

Into various liquids in succession at 67° F. a cylinder of metal at 212°

main, at each experiment, for 15 minutes. The liquid to be examined, and all the apparatus (but the iron bar), were, at each experiment, ascertained to be at 67°. The liquid was poured into the wooden vessel, till it could rise 0.1 inch on the side of the iron cylinder when in its place: The wooden top was put on, and the iron was drawn out of the kettle of boiling water by means of the attached string, and instantly let down through the hole of the cover. The time the thermometer took to rise through three degrees (to 70°) was accurately marked by means of a stop-watch, and the results of my experiments on several fluids are exhibited in the following

and the rise of thermometer 0.5 inch beneath, was observed in three minutes.

TABLE.

Table of results.

	Liquids.	Minutes.	Seconds.
1.	Water, - - - -	7	5
2.	Milk of a Cow, - -	8	25
3.	Proof Spirit, - - -	8 nearly	
4.	Alkohol. London Pharm.	10	45
5.	Transparent Olive Oil, -	9	50
6.	Mercury, - - -	0	15
7.	Solution of Sulphate of Iron, one part of Salt to five of Water, - - -	8	0
8.	Saturated Solution of Sulphate of Alumine, -	9	40
9.	Ditto Solution of Sulphate of Soda, - - -	6	30
10.	Aqua Potass. Puræ. Lond. Pharm. - - -	8	15
11.	Saturated Solution of Sulphate of Soda, but the Liquid not touching the Iron Cylinder by 0.1 Inch, or *nearly so*, -	19	20

As the water in the first experiment was employed at a temperature above 42°, it could not affect the thermometer by any change of density; it may therefore serve as a standard to compare the other liquids. With regard to the differences of a few seconds, we need not insist on it as indicating any material difference between the conducting power of the different substances; because the eye may not be able to mark it instantaneously: but where this difference amounts to nearly a quarter

The temperature was always too high to produce a descending current in water by heating.

a quarter of a minute, much more when to several minutes, we may fairly conclude, that there is a difference in conducting power.

In all these experiments the sides of the apparatus should have produced equal increments, had this been the cause of the rise of the thermometers, and it is evident that currents downwards could not affect it. That the sides of the vessel could not communicate the temperature to the thermometer, nor even the radiant caloric affect it in the manner observed, the eleventh experiment (which by the way arose from an error in the mode of conducting the trial with sulphate of soda) sufficiently demonstrates. From an inspection of the table, it will be seen, that the aqueous solutions of different salts differ materially from each other in the celerity with which caloric is propagated through them.

I attempted to measure the conducting powers of several of the weaker acids, but I was soon convinced that their action on the iron might invalidate the accuracy of the results.

The fluids are proper conductors.

It will be unnecessary to observe that if we find the thermometer requiring different times for its elevation, in such cases, we must ascribe it to the conducting powers of the medium between it and the heated body.

If I am not deceived, we may conclude from what I have above adduced, that liquids as well as solids are conductors of caloric; that the transmission of it through them follows a particular law depending on the properties of the particular liquid, but which is not in the exact ratio of any of their mechanical properties, though nearer that of their *density* than any other.

The Count's facts may be as well explained by the slow conducting energy of fluids as by its negation.

Very slow currents will explain his fact of the Glaciere of Chamouni.

Such, Sir, are the principal arguments that seem to militate against Count Rumford's hypothesis, which he has, with that ingenuity which distinguishes his researches, applied to the solution of many important phenomena of nature. These, however, may be equally well explained by supposing liquids very bad conductors of caloric; and, if the currents caused in liquids by changes in temperature, have even a very inferior velocity to what he supposes, we may, I think, account sufficiently well for the appearance he observed on the Glaciere of Chamouni, which he proposes as a test of his opinions, by the decrease in density of water while its temperature descends from 42° to 32°, (a fact which the Count's late experiments confirm) without assenting to his opinions with regard to

to the non-conducting power of fluids. An examination of this would, however, extend farther this already too long letter; but if you deem such an enquiry interesting, it may be the subject of a future communication.

I am, Sir,

Your's with respect,

T. S. TRAILL.

*** As this letter did not come to hand till above a fortnight after its date, and the verbal description is very clear, it was not thought necessary to postpone it for engraving the author's sketch.—N.

XIII.

Indian Account of a remarkably strong and ferocious Beast, which (they say) existed in the northern Parts of the State of New York about two hundred Years ago. Collected and communicated by Mr.* JOHN HECKEWELDER.

THE jagisho † (or naked animal, or bear, as some of the Indians call it) was an animal much superior in size to the largest bear. It was remarkably long-bodied, broad down its shoulders, but thin, or narrow at its hind legs, or just at the termination of the body. It had a large head and a frightful look. Its legs were short and thick. Its paws (the toes of which were furnished with long nails or claws, nearly as long as an Indian's finger) spread very wide. Except the head, the neck, and the hinder parts of its legs, in all which places the hair was very long, the jagisho was almost naked of hair; on which account the Indians gave it the name of "naked." Account of the large animal called jagisho by the American Indians.

Several of these animals had before this time been destroyed by the Indians, but this particular one had, from time to time, destroyed many of the Indians, particularly women and children, when they were out in the woods getting nuts, digging roots, &c. or when they were working in the fields. Hunters when fast pursued by this animal, had no means of escaping from

* To the Editor of the Philadelphia Medical and Physical Journal, whence this is taken.

† The Indian name of this beast or animal.

Account of the large animal called jagisho by the American Indians.

from it except where a river or lake was at hand, by plunging into the waters, and swimming *out*, or *down* the stream to a great distance, they effected their escape. When this was the case, and the beast was not able to pursue his intended prey any further, he would set up such a roaring noise, that every Indian who heard it trembled with fear.

This animal preyed upon every beast it could lay hold of. It would catch and kill the largest bear, and devour it. While the bears were plentiful the Indians had not so much cause to dread the jagisho; but when this was not the case, he would run about in the woods, searching for the track or scent of the hunters, and follow them up. The women became so much afraid of going out to work, that the men assembled to deliberate on a plan for killing him.

This beast had its residence at or near a lake, from which the water flows in two different ways (or has two different outlets), one northerly and the other southerly. The Indians being well informed of this circumstance, a resolute party of them, well armed with bows, arrows, and spears, made towards the lake. They stationed themselves on a high perpendicular rock, climbing up the same by means of Indian ladders, and then drawing these ladders up after them.

After being well fixed, and having taken up with them a number of stones, the Indians began to imitate the voices and cries of the various beasts of the woods, and even those of children, in order to decoy the jagisho thither. Having spent some days in this place without success, a detached party took an excursion to some distance from the rock. Before they had reached the rock again, the beast had gotten the scent of them, and was in full pursuit of them. They, however, regained that position before he arrived. When he came to the rock, he was in great anger, sprang against it with his mouth wide open, grinning and seizing upon it, as though he would tear it to pieces * * * *. During this time, numbers of arrows and stones were discharged at him, until at length he dropped down and expired.

His head was cut off, and was carried in triumph by the Indians to their villages or settlements on the North River, and was there fixed upon a pole that it might be seen. As the report of the death of the animal spread among the neighbouring

neighbouring tribes, numbers of them came to view the head and to praife the victorious Indians for their warlike deed.

Account of the large animal called jagifho by the American Indians.

N. B. The Mahicanni claim the honour of this act.

Remarks by the Editor.

The preceding traditional accounts of the Indians, concerning the "naked beaft," are in fome refpects, fo very extravagant, that they may perhaps be deemed altogether unworthy of any attention. I muft confefs, however, that I cannot but confider fuch traditions, though imperfectly handed down to us, and evidently disfigured by fable, as entitled to the notice of the naturalift and philofopher.

That fuch an animal as the Jagifho is defcribed to have been, has ever exifted in the ftate of New York, may perhaps admit of a rational doubt; but that the Indian tradition relates to *fome remarkable* animal that is no longer to be feen in the country which it is faid to have inhabited, I think there is good reafon to believe. What this animal was, at what period it ceafed to be feen, and what was the more pure account of the Indians concerning it one hundred years ago, I do not pretend to determine.

Poffibly the Indian tradition refers to the large animal, (I mean an individul of the fame fpecies,) fome of whofe bones have been found in a cavern in the back parts of Virginia; the animal of which mention is made in the firft part of this Journal*. Is is true indeed that the Indian accounts of the activity of the New York animal are not very favourable to the idea, that the animal was Mr. Jefferfon's Megalonyx, which I have fuppofed belonged to the order of Tadigrada, comprehending the Sloth, the Armadillo, and others. What is faid of the claws of the Jagifho may be thought to favour the notion that this was really the Megalonyx, or Megatherium. But I would not be underftood to place any dependance upon the minute or defcriptive circumftances which are mentioned in the Indian tradition. Nor indeed do I think it at all probable that the Megalonyx (as it is called) or any of the fpecies of elephants whofe exuviæ abound in various parts of North America, have been feen in a living ftate in this Continent, within the period of two, or even twice two hundred years.

* Section Third, p. 152, 154.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Death of Professor Claproth.

Klaproth.

SOME of the foreign Journals have announced the death of the celebrated Klaproth of Berlin, who, for the benefit of the Sciences, continues in good health in his sixty-second year. Mr. Justus Claproth, Professor of Jurisprudence in the University of Gottingen, well known for several learned works on that subject, died on the 10th February last, in his seventy-seventh year.

Astronomical Prize.

Lalande's prize given to Harding.

The medal founded by de Lalande for the best astronomical work, has been adjudged by the National Institute in its sitting of April last, to Mr. Harding, for his discovery of the last new planet. That able astronomer has been appointed to the direction of the Observatory at Gottingen.

New Musical Instrument.

New musical instrument.

A Polish clock-maker named Masloufky, arrived at Berlin, at the beginning of the present year, with the intention of exhibiting a new stringed instrument, of his invention. Notwithstanding a variety of advertisements, he did not succeed in attracting the public notice; and he determined to exhibit the instrument at a concert previous to his departure. About 300 auditors attracted by the names of Himmel and Seidlen, who were to perform, attended, and towards the end of the concert nearly half the number retired. The artist proceeded to exhibit his Kœlison, which is the name he gives the instrument. It consists of a sound-board, on which the usual system of wires of the piano are fixed. Between these wires are small wooden cylinders, which being put into motion, communicate their vibrations to the wires. The tones are so soft and enchanting that the harmonica cannot equal them, the forte and piano are given in every imaginable gradation, and the whole effect was no less surprising than unexpected, and the maker Huhn received orders for a number of the instruments.

The

The present article is taken from Millin's *Magasin Encyclopedique*, who does not say whether the wooden cylinders were moved in rotation or otherwise, nor how they were applied and pressed against the strings. The leading fact of this notice seems to be, that there are certain kinds of wood, and perhaps certain resinous or other matters to be applied to them, that will produce the effect of a bow upon wire strings in a superior manner. It is indeed probable, that we do not yet possess much knowledge of the art of producing tones by the powerful expedient of bowing, or light friction; and mechanics have still an ample field for applying this method with force, precision, and rapidity to the more compounded instruments.

Saverien.

On the 28th of May last died Alexander Saverien engineer of the French marine; who has been sixty years known to the scientific world, for his writings on navigation and the theory of building, rigging and manœuvering ships. He has written accounts of the instruments for making observations at sea; a marine dictionary; a dictionary of the mathematics; a dictionary of architecture; an history of modern philosophers, and an history of the progress of the human understanding. His works indicate a considerable share of talent and very extensive knowledge. For many of the last years of his life he was poor and infirm, and was much indebted to the cares of a servant who continued with him from motives of attachment. He died at the age of eighty-five, leaving behind him a widow likewise very aged and in want. Death of Saverien.

Pure and beautiful Ceruse.

Mr. Van Mons informs us, that, if lead ashes be dissolved in a sufficient quantity of dilute nitric acid by the help of a gentle heat, filtered, and precipitated by chalk in impalpable powder, the precipitate, when washed and dried, will be the purest and most beautiful ceruse possible. The question which offers itself on this occasion, is whether it could be afforded at a reasonable price. Pure and beautiful ceruse.

Chromate

Chromate of Lead and of Silver.

Chromate of lead and of silver dissolved in nitric acid.

Count Mouffin Pouschkin has dissolved both the red lead spar and chromate of silver in nitric acid, by adding a little sugar the moment the acid is poured on, and promoting the action by gentle heat. The spar then requires only five or six parts of acid, the chromate of silver still less. Nitrous acid gas is evolved, and the solution of the former is an amethyst colour, of the latter a garnet red, without the least trace of green, either by reflection or refraction.

Putrefaction prevented.

Putrefaction prevented by red precipitate,

Dr. Valli having left a pound of soup in which were twelve or fifteen grains of red precipitate, exposed to the open air for four months, found it exhibited no sign of putrefaction, and did not even seem to have undergone any alteration. He then repeated the experiment for a month in the height of summer, with the same effect. The oxide in the mean time had neither diminished in weight nor altered its colour.

without loss of weight or colour; and by the combination of oxigen.

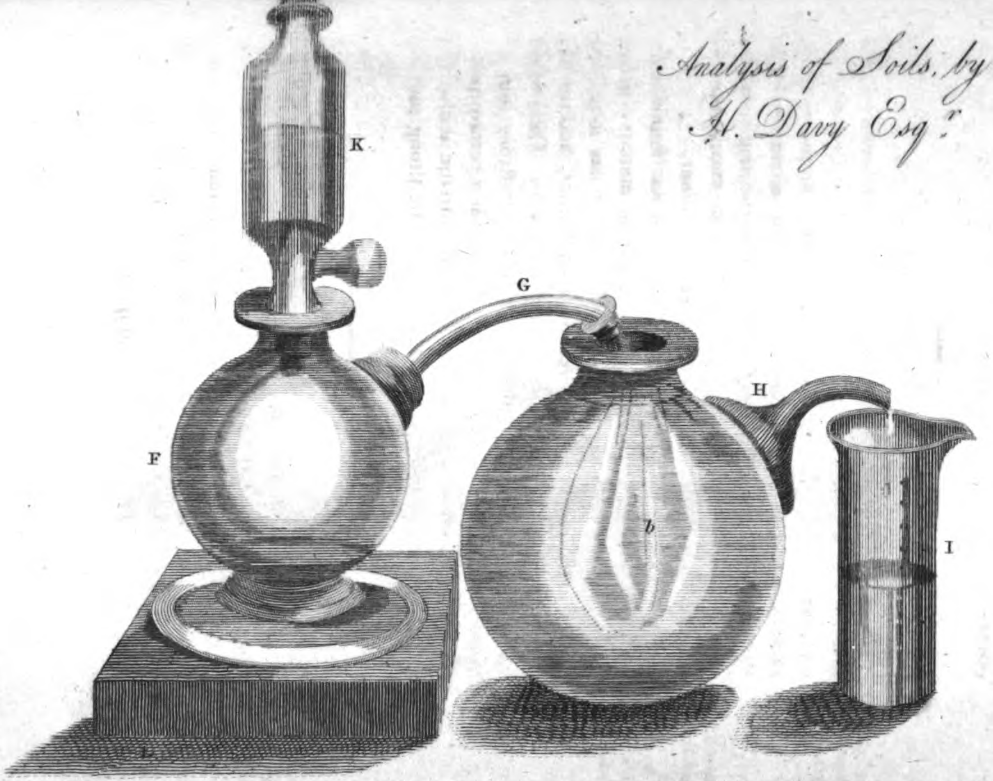
On this Mr. Van Mons observes, that he has found broth keep for years by means of a few grains of mercury in the state of oxide and citrate. Nitrate of silver has long been considered as the most powerful of antiseptics, and he has found those of gold and of mercury equally so. Oxigenated muriate of potash retarded the putrefaction of strong soup several days, and ultimately put a stop to it at a certain point. Very dilute nitric acid, and oxigenated muriatic acid in the state used for bleaching, preserved a moderate strong soup for several months.

Leverian Museum.

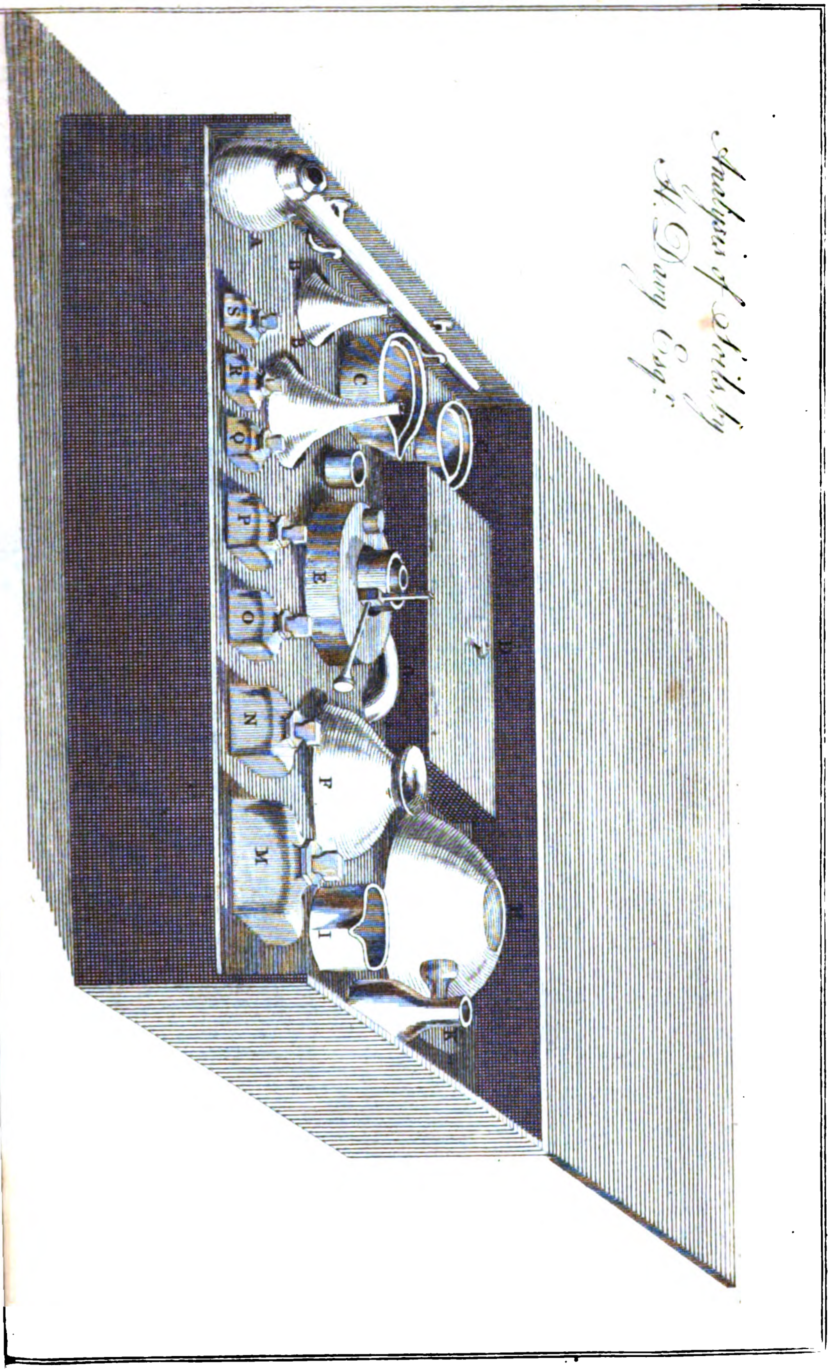
Sale of the Leverian Museum.

The Leverian Museum which has been near 40 years in collecting at an expence of near 50,000*l.* will be sold by public auction in May 1806, unless an acceptable offer for the purchase of the whole be previously made. The sale will take place in the building which now contains the Museum. Catalogues are preparing with all speed.

Analysis of Soils, by
H. Davy Esq.r



Mutlow Sc. Russell Ct.


Analysis of Soils, by
H. Davy Esq.

Count Rumford's Investigations concerning heat.

Fig. 1.

Fig. 2.

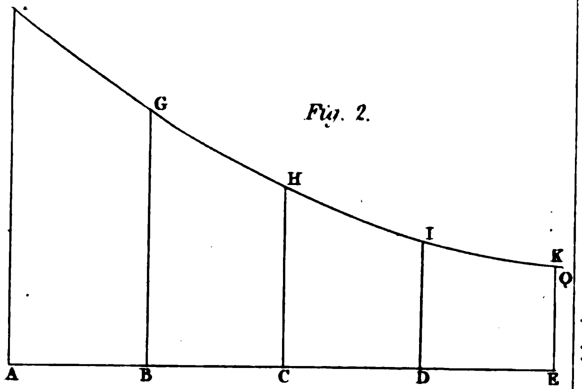


Fig. 3.

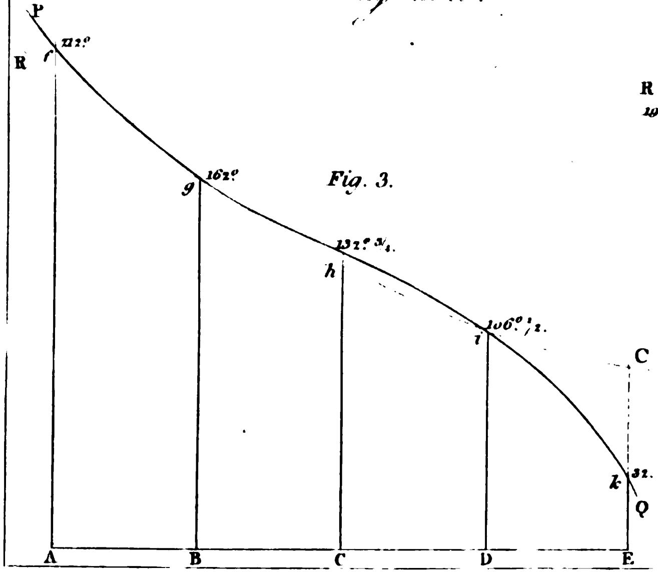
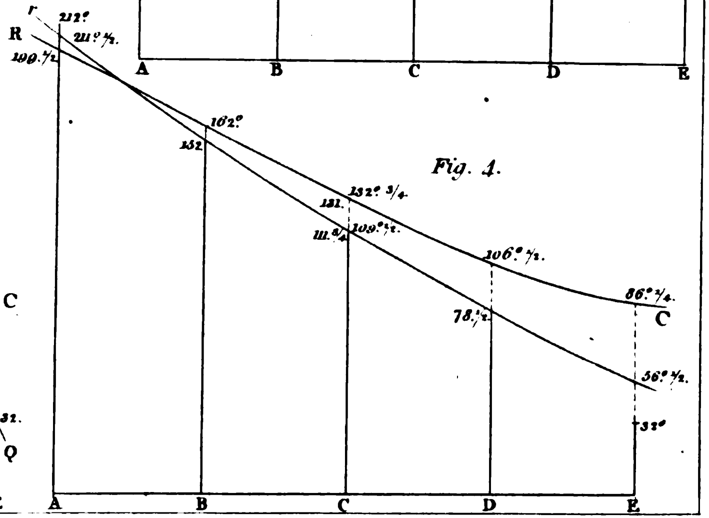


Fig. 4.



Mutlow Sc. Russell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1805.

ARTICLE I.

Facts and Observations relating to the Blight, and other Diseases of Corn. In a Letter from G. CUMBERLAND, *Esq.*

To MR: NICHOLSON,

SIR, *Weston-supra-Mare, 10th October,* 1805.

ALTHOUGH not much in the habit of taking any thing on trust, I must confess, that when early in the spring the pamphlet of Sir Joseph Banks, called, *A short Account of the Cause of the Disease in Corn, called by the Farmers, the Blight, the Mildew, and the Rust**, came to my knowledge, I felt very much inclined on the credit of his extensive fame to receive with a favourable prejudice, what was so positively announced as information, on a subject that we may all, I suppose, be allowed to have something to do with. Reference to the Pamphlet of Sir Joseph Banks on the blight in corn.

Having therefore much leisure, and being placed in a favourable situation to make observations, I began doing all that could then be done, viz. planting some wheat near a barberry bush, and searching carefully for the yellow rust on the early leaves. Corn sowed near a barberry bush.

* Inserted intire in our Journal X. p. 225.

Diseased straw being examined, it was seen that Mr. Bauer's drawings are very correct.

I next examined the straw by the microscope, making many dissections with great attention. The result of which was, a conviction that the designers and engravers part of the fine plates that accompany this essay, has been excellently and faithfully managed, as far as the then season would permit me to compare them, and some old straws of the last year rendered it very probable, that the whole was correctly drawn.

Doubt whether this disease affects the grain.

But here I began to suspect, that the *rust* was not so guilty as has been represented, for, admitting for a moment, that it does intercept the sap by plugging the apertures said to be destined in wet weather to receive the humidity of the atmosphere, yet, as it is *not yet ascertained* that it strikes root, into the cellular texture beyond the back, for I could not admit of saying there is *no doubt* of a thing that *has not yet been traced*, see page 11 (or 227 of Journal) and as the plates, if they prove any thing *prove the contrary*, *vide* fig. 7. I thought we must wait awhile before we could charge to this cause the diminution of our flour. There was yet another motive in my humble opinion for doubt: I saw, even by examining the straws of last year, that it was scarcely possible to find among hundreds of *rusted straws* that had *blighted heads*, one that any way partook of the rust except at the upper joint, and that partially only, while the sheaths that nature has kindly given to ward off injuries, were compleatly consumed with it. Now I believe, no one will pretend to assert, that this injury done to the sheaths of the straws could in any way affect the rising of the *sap* to the ear; we must therefore, I soon saw, confine the cause of injury, *if this be any cause*, to the quantity of fungi that more immediately attacks the upper bare joint of the straw.

And even here it appeared at this early stage of my doubts to be very uncertain; for if we reflect first of all that it is by no means pretended to be proved, that these fungi do penetrate the cellular texture; and next, that if they do, it will remain to be proved, that by so doing they materially intercept the sap; and lastly (which I conceive to be no extravagant conjecture) that if they did, yet as far as the cellular texture of the straw is concerned in conveying it, the interception could only be very partial.—Taking all these reflections together, I think I was grounded in entertaining doubts of the true *cause* having been exhibited, as is set forth in this pamphlet, p. 13.

p. 13. (or 223 Journal.) Where the president says, "though diligent enquiry was made during the last autumn, no information of importance relative to the origin or the progress of the *blight* could be obtained! this is not to be wondered at, for as no one of the persons applied to had any knowledge of the *real cause* of the malady, none of them could direct their curiosity to the real channel. *Now that its nature and cause have been explained*, we may reasonably expect that a few years will produce an interesting collection of facts and observations, and we may hope that some progress will be made towards the very desirable attainment of either a preventive or a cure." page 14.

Having thus advanced in the examination as far as the season would afford, I thought it would be best to ascertain in the county in which I happened to be placed, the terms (intirely overlooked in the pamphlet) there applied to the different diseases of corn; and here it soon appeared that the terms are not universal. **Country names of the diseases of wheat.**

The first blight (for there are many) is that early appearance of intirely decayed ears, of plants apparently in a healthy state, but which, in the embrio which lays within the upper sheath, before the ear is develloped, has turned to a brown puce-coloured powder. This by some has been supposed to originate from defective seed, but surely improperly; for many of these ears are found to be the finest and largest in the field in their embrio state; and to me it seems evident, that they are imperfect from a really defective parturition, owing to some accidental circumstance; or possibly (for they generally stand below the others) from the want of sun to unclose the upper sheath at a critical moment. **First blight; or mildew, by which the embrio grain is turned to powder.**

These ears are also in general found to be crooked at the root of the ear stalk, owing to the effort to raise the ear acting within the sheath on a decayed and mouldy ear; but it appears that they are all at length ejected from the sheath, and the brown dust blown compleatly away, so that the stalk at last remains without a single grain on it, standing up like a bare and barren pole. In this manner many ears at first perish, but the quantity is seldom any object to the farmer. **The ears are usually crooked at bottom, &c.**

Of the puce-coloured powder a quantity was collected: it had no smell, but felt soft to the touch, like whiting, though more greasy. An attempt was made to innoculate a number **Appearance of the powder; it produces no infection.**

 of

of other found ears with it; by rubbing it on the leaves, by giving friction to the ftraw itfelf with it, by inferting it into the ears, by placing it beneath the fheaths, and laftly, by introducing it into the pith of feveral ftraws,—each of thefe in wet and dry weather, but nothing took any effect. It fell harmlefs:—but one difcovery arofe from examining the ears in which it was produced, viz. that corn is capable of being completely blighted without any external difeafe or application. For all the ftraws were without blemifh as well as the leaves, and confequently we have no occafion to recur to external caufes for this internal decompofition.

The firft blight does not arife from an external caufe.

Wheat in this ftate I drew very accurately, and fent fpecimens of it to town in this condition to Mr. Nicholfon; and thus ended the firft blight as it is called in Somerfetfhire, but which might poffibly be with more propriety termed the *mildew*.

Second difeafe. Smut. Full grains, containing only a dark powder of a fifhy fmell.

The next difeafe appears in thofe ears which ftand erect and ftaring, indicating their lightnefs by their attitude. Although on gathering they appear full of corn, they turn out in effect to be full of a dark powder that has the fmell of ftale lobfters or fhrimps, when preffed between the fingers. This powder on examination by the microfcope fhews fome faccharine concretions among it, but it has none of the actual properties of wheat. Upon carefully examining thefe grains the outer fkin was difcovered to be *intire, unperforated, perfectly green, and perfectly full*;—yet ftrange to tell, if it be not really the work of infects, one half of an ear was often found to be thus fmutty while the other half was found.

Mechanical injury did not produce this difeafe.

Among other conjectures, it was thought that this fmutty difeafe might arife from the juices of the ftraw being intercepted by accidents. In order therefore to try what could be effected by injuries done to the fap, I bound fome ears, wounded others by preffure, divided fome with a knife near the ftem, marking each by cutting off the beards with fciffars; yet I never found any fuch effect produced, as either fmut, or even decay, and all the ears thus injured came very well to maturity. Here were real injuries, and committed at a time (June) when the ears were by no means far advanced, and this led me ftill lefs to expect any great effect from a little partial mofs adhering to the ftem, the accidental effects of the feafon at a later period.

At length on the 1ſt of Auguſt, I ſaw in a low field the firſt appearance of yellow ruſt, but collected one with difficulty; and now I found that it was univerſally agreed by our farmers, that this fungus, as our plate deſcribes it, was conſidered as the effect of fogs or great humidity, which firſt attacks the leaf or ſheath of the ſtraw, but ultimately penetrates, and vegetates on the upper joint of the ſtraw itſelf, where it is uncovered juſt before the corn is ripe, ſo that what our plate exhibits is by all agreed to be called the *ruſt* in all ſtages; but that the ruſt cauſes the *latter blight*, or ears with ſhrivelled grains does not ſeem to be ſo generally agreed.

Third diſeaſe, the ruſt conſidered as effect of humidity.

To prevent the *ſmut*, our farmers ſteep their wheat in ſalt water, in order to ſeparate the ſound grains from the light or blighted better than by freſh water, becauſe ſalt water naturally floats all but the heavieſt. In the *Venetian* ſtate I remember they added ſaltpetre to the ſteep on the ſame principle, a very different ſyſtem from that of Sir Joſeph, who recommends, I think very dangerouſly, the uſe even of tailings as ſeed, and this on mere hot houſe experience, ſee p. 25 (or 232 Journal.)

The proceſs of ſteeping grain is uſed to ſeparate the heavieſt for ſeed.

An old and good farmer* laſt year at *Weſton*, bought good wheat for ſeed becauſe his own was *ſmutty*, but not having quite enough, he ſowed about three pecks of his own ſmutty wheat to finiſh, and it turned out quite as good wheat as ſome of that which was bought as the beſt; yet this does not confirm the doctrine, I think.

Smutty grain not uſed for ſeed, yet ſome ſucceeded.

Having thus aſcertained what is called the early *blight or mildew*, together with that which follows, and is known by the term *ſmut*, and alſo the diſeaſe which comes next, and being ſimilar to Sir Joſeph's Banks's plate, is called *ruſt*, and laſtly the *latter blight* which is ſeen in merely ſhrivelled grains, or grains imperfectly ripened;—I ſhall now proceed to the ſpecimens which I gathered in all the ſtates, made drawings of them, and ſtill retain in the ear labelled with great care, as proofs of what is here advanced; after which I ſhall make ſome deductions.

Latter blight; or ſhrivelled grains.

On 22nd of July, 1805. I began my collection; and No. 1. contains healthy ears, clean to outward appearance, the loweſt leaf a ſpeck or two of fungus. Second joint from the head a little reddiſh. No bloom on the cane.

Accounts of ſpecimens of wheat gathered in this enquiry.

* Mr. Oakley of Weſton-Supra-Mare.

No. 2.

Accounts of specimens of wheat gathered in this enquiry.

No. 2. Much diseased straw, bloom or mould under the sheath, and on the leaf; in other respects the straw quite as healthy and sound as No. 1.

No. 3. Entirely diseased, yet every grain full and of its proper size—some grains evidently opened at their sides by some small insect. The outside of this ear quite green and healthy, not even a spot, smell of the ear very fishy, not wanting one grain. The head stalk not even waved at the ear, root intire, upper stalk a little yellow.

No. 4. A healthy ear of the bearded, thirteen rows of grains, straw mouldy under the sheath, and at joints, yet sound, last or upper joint green.

No. 5. Another nearly similar.

No. 6. A double straw to one root, both ears perfect, both straight at head stalk, both diseased, yet full and plump; some grains sound yet green, and close to a black one; a rich golden coloured moss or dust at the back of the green coat, yet the grain coat perfectly green and uninjured, while the grain was compleatly full of the black fishy-smelling blight instead of flour. Straws green at the top and quite healthy throughout.

No. 7. A straw that having been blighted in the sheath by the early blight, had thrown off all its brown dust, and grown to a strong straw: The skeleton of the ear only remaining, very crooked near the ear: The upper stalk evidently by its purple stripes diseased under the skin, yet no moss or fungus protruded on the cane; stripping back the first leaf from the head, I found the powder from the ear had adhered to the straw under the sheath, and that it was mouldy: The second joint quite healthy under the leaf, but with the red and purple streaks where uncovered to the light: The third joint the same: The sheath leaves themselves healthy.

No. 8. Spotted ear externally, sound straw, yet having black dust at the joints, in this ear I found a maggot about the 30th part of an inch long, of the exact yellow of the powder found behind the diseased grains, (that powder may be his excrement) viz. orange yellow, his form resembled the maggot of nuts, lived an hour on the table.

No. 9. Diseased ear, grains all blighted yet no yellow powder under the sheaths of the grains, straw healthy, ear fishy smell.

No. 10.

No. 10. Ditto with an inſect very active in the ear, yellow, ſee drawing A B. Fig. 1. Plate IX.

No. 11. Two perfectly healthy ears from the ſame root, both healthy throughout, yet on the leaf a ſpeck or two of Sir Joſeph's fungus was originated, and well grown: it was of the orange colour.

No. 12. An ear faſt ripening, ſolid in the grain, yet had loſt its firſt ſix lower grains, laſt joint green, beards yellow, a few ſpots on the ſheath of the grain, ſeemingly occaſioned by a ſmall black fly found in it.

Obſervations made on the ſame Day on Grains.

Diſeaſed grains were always found to be full and plump without any aperture; the fine ſkin that holds the flour very green, yet all black within; the external ſurface of the black matter covered with a white concretion, perhaps the ſaccharine matter of the wheat. Sound grains found in diſeaſed ears, all the diſeaſed ears ſmelt filthy. Diſeaſed grains deſcribed.

Obſervations made on the Straw.

The diſeaſe always attacks the portion of the ſtraw that peeps beyond the ſheath leaf near the joint, and evidently commences at the pores, as old ſtraws will ſhew; but the fungus cannot, I think, be the cauſe of the diſeaſe; becauſe where no fungi have taken root, the corn is compleatly corrupted. Theſe fungi grow it is true, faſteſt on diſeaſed ears, probably becauſe when the ear is diſeaſed it draws leſs humidity from the ſtraw, ſo that the diſeaſed ears ſeem to aſſiſt the growth of the fungi, not the fungi the diſeaſe. In fact, I believe they live on the ſuperfluous moiſture of the ſtraw, or returning ſap. Diſeaſed ſtraw.

On the 27th July laſt, 1805. I examined at leaſt twenty blighted ears of corn, and could only find one among them that had the ſmalleſt degree of *ruſt*, and that only a ſpeck or two on the lower leaf that ſheaths the third joint. The ſpots were orange colour and deep purple, and did not occupy half the diameter of a ſpace of three inches long. Blighted ears, the ſtraw not ruſted.

Upon again examining many *healthy plants*, I found not only their ſheath leaves, but even the ſtraw eaten away by ſome Healthy plants having the ſtraw injured.

same insect, and at the same time discovered abundance of the green locust like insect on the ears; of which, see the magnified design, Fig. 2. Plate IX.

Inoculated plants, and others injured by violence, took no disease.

Some plants that I had lately innoculated were still sound: others that I had pinched and bruised in the upper joint shewed no alteration, but in all respects resembled the most healthy.

Plants gathered which have the straw and the grain not in the same (healthy or diseased) state.

Aug 3rd, 1805, I went to a field of Mr. Oakley's, o Weston, where the wheat had a general good aspect; and selected and labelled, of ears green yet full.

No. 1. Sound ears with diseased straws.

No. 2. Compleatly smutty ears with sound straws in every respect.

No. 3. A smutty and a sound ear from the same root. The sound ear had a speck of fungus.

No. 4. Ears with crooked tops, others twisted by spiders; others with crooked beards, short stalks and long; yet all of them full of grain, green and sound.

No. 5. Ears half smutty, viz. on one side all the way up.

No. 6. Ears half stagged, (stagged means here those that shew only the skeleton of the ear and the crooked upper stalk from early blight, all but the bare poles being blown away) these ears were half *stagged* and half covered with grains.

No. 7. Bare *staggs*, but with quite fine sound straw.

Other instances of sound wheat with rusty straw; and shrivelled wheat with sound straw.

Lastly; in August and September, 1805, at Allcombe near Minehead, I collected out of a field just reaped, two bundles which with all the others I still keep as proofs of my assertions: One all of sound wheat with all their upper stalks very much covered with the rust of Sir Joseph Bank's description, and the other all of black and shrivelled ears, yet all sound in the upper stalk.

The shrivelled corn was never fairly ripened.

These latter mentioned stalks, I think, threw great light on my ultimate conjectures drawn from every observation through the whole season; viz. they present shrivelled, blighted grains, and exhibit short ears, because on examination they were evidently never sufficiently exposed to light and heat: for all their straws though clean were green, not yellow as those of ripe wheat ought to be, and their smoaky miserable appearance (not having the least smell of the fishy smutt) could only arise from their humble situation below the other ears, where air, sun, and light was deficient; in fact, they never ripened properly. The straw remained green, and the sap probably

probably returned inſtead of being intercepted by the drying of the upper joint; and to me it now appears to be a fair conjecture, that what is generally called blighted corn, or thoſe ears that produce ſo many ſhrivelled grains, and which we are called upon to ſeek a remedy for, is nothing more than the effect of a practice of late much recommended, viz. *to reap early*, a practice not only promoted by the *Miller*, who is eager for the new corn to come to market, but by the avarice of the *Farmer*, who fears that by letting it ſtand too long the grain may fall in price, and reduce his profits; and, what is ſtill more unfortunate, by ſome agricultural writers of great reputation, who recommend it as ſtopping the progreſs of the ruſt, forgetting that the ſun only can effectually deſtroy that ſuppoſed evil, by well drying the ſtraw.

What is called blighted corn ſeems to be corn prematurely reaped, from avarice or ignorance.

Far be it from the writer of theſe few remarks to diſcourage any attempt at ſaving in a remarkably wet ſeaſon, or in remarkably wet ſituations, wheat that has paſſed the period uſually productive of ripe corn. He knows that in caſes of laid-wheat in ſhady ſituations, by reaping it early, we may accelerate the ripening of that which otherwiſe would not have ripened at all, by the operation of turning and expoſing the ſheaves to the ſun, and ſo make good ſaving crops; but what he wiſhes to guard againſt is, that eagerneſs for putting in the ſickle originating in the motives before alledged; for, reaſoning from analogy do we not always find, that in all other ſeeds that are to be gathered, theſe alone are plump, ſound, and full of their proper flour, that are ſuffered to receive the utmoſt influence of the ſun while on the ſtalk; and he always thought many years ago, that we were in the habit of being too fearful of the latter ſeaſons; it is true that late harveſts are expenſive in collecting, but they are generally well matured, and the inſtance of *barley* that may be well ſaved (for colour can have little to do with the intrinſic value of grain) even as late as November, proves the juſtice of the obſervation; fruit gathered too ſoon, diſappoints all views of profit or pleaſure, and we might, he thinks, as well attribute the ſhrivelling of our apples early gathered to the influence of the apple-tree moſs, as the ſhrivelling of our grains of wheat to a ſuppoſed blight originating in the funguſes, that have of late ſo much alarmed the theoretical agriculturiſts, and œconomiſts of our day.

It is often adviſeable to reap early.

Argument in favour of late reaping.

On

Conclusion.

On the facts exposed I could greatly enlarge, but I think on all accounts it is best at present to offer them in their present form to the reasoning faculties of your readers. At any rate they may serve as data, and if they should fail to bring others to my opinion, may act, I trust, as useful stimulants to the further investigation of a very curious subject of enquiry, as to what are the nature of the enemies to the perfection of our wheat harvest.

With respect and esteem,

I am, Sir,

Your most obedient humble Servant,

G. CUMBERLAND.

The vicinity of the barberry does not seem to affect grain.

P. S. I ought to observe, that on the grains of wheat sown near the barberry, I had no opportunity of making observations; but that I have a dried root of wheat now by me, on which there are above 100 straws that are all clean and sound, though it grew a few years ago in a garden where barberry bushes were.

II.

Experimental Investigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. &c.*

(Concluded from Page 75.)

SECT. III. *Experiments tending to shew that Heat is communicated through solid Bodies, by a Law which is the same as that which would ensue from Radiation between the Particles.*

Object of inquiry; the laws of the propagation of heat in solids.

HAVING made a considerable number of experiments on the passage of heat through fluids, and through different substances in the state of powder, I was curious to ascertain the laws of its propagation through solid bodies, particularly metals.

I hoped this discovery would furnish some additional data, to confirm or refute the opinions I had adopted concerning heat and its manner of acting; and it will be seen by the results, that my expectations were not frustrated.

Having

Having procured two cylindrical veſſels of tin, each ſix inches in diameter and ſix inches high, I faſtened them together by means of a ſolid cylinder of copper ſix inches long and an inch and half in diameter, which was fixed horizontally between the two tin veſſels. The extremities of the cylinder paſſed through two holes an inch and half in diameter, made for the purpoſe in the ſides of the veſſels, midway between the bottom and top, and were ſoldered faſt in them.

Deſcription of an inſtrument: Two cylindrical tin veſſels were connected by a bar of copper.

Each of the veſſels was made flat on the ſide where the copper cylinder was faſtened, ſo that the extremity of the cylinder did not project into the veſſel, but was level with the flattened part.

This inſtrument was ſupported at the height of eight inches and half above the table on which it ſtood, by means of three feet, two fixed to one of the veſſels, and one to the other.

One of theſe veſſels being filled with boiling water, the other with water at the freezing point; as the two extremities of the cylinder were placed in immediate contact with theſe two maſſes of fluid, a change of temperature muſt neceſſarily take place by degrees in all the interior parts of the cylinder. For the purpoſe of obſerving this change, three vertical holes were made in the cylinder, into which were introduced the bulbs of three ſmall mercurial thermometers. One of the holes was in the middle of the cylinder; the others midway between the centre and either extremity.

The veſſels filled with water, one at 212°, the other at 32°.

The changes marked by three thermometers at equal diſtances.

Each of theſe holes is four lines in diameter, and eleven lines and half deep; ſo that the bulbs of the thermometers, which are three lines in diameter, were all in the axis of the cylinder.

When the thermometers were put in their places, the holes were filled with mercury, in order to facilitate the communication of heat from the metal to the bulb of the thermometer.

To keep the hot water conſtantly boiling, a ſpirit lamp was placed beneath the veſſel containing it; and to keep the cold water conſtantly at the temperature of melting ice, freſh portions of ice were added to it from time to time.

The one water kept boiling by a lamp; and the other cold by addition of ice.

The thermometers are graduated to Fahrenheit's ſcale, the freezing point being marked 32°, and that of boiling water 212°.

As the firſt and moſt important object I had in view was, to learn at what temperature the three thermometers would become

The thermometers not noticed till nearly ſtationary.

come stationary, I did not very carefully notice the progress of the thermometers toward this point; but as soon as they appeared nearly stationary, I observed them with the greatest attention for near half an hour.

The thermometers distinguished.

To distinguish the three thermometers I shall call that nearest the boiling water B, that in the centre C, and that nearest the cold water D.

Experiment.

The following are the progress and results of an experiment made the 28th of April, 1804, the temperature of the air being 78° of Fahrenheit.

Tabulated results.

Time.			Temperature of the hot water.	Temperature marked by the thermometer B.	Temperature marked by the thermometer C.	Temperature marked by the thermometer D.	Temperature of the cold water.
H.	m.	s.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
1	52	15	212	160	130	105	32
—	53	30	—	160½	131	105½	—
—	55		—	161	131¾	106	—
—	56	30	—	161¾	132	106½	—
—	58		—	162	132½	107	—
2	0	0	—	162	132¾	107½	—
—	1	30	—	162	133	107½	—
—	4		—	162	132¼	106½	—
—	6		—	162	132	106	—
—	9		—	162	132¼	106½	—
—	11		—	162	132¼	106½	—
—	28		—	162	132¼	106½	—

Account of the results that would have followed the hypothesis of heat being propagated through bodies by radiation from particle to particle.

Before I proceed to examine more minutely the results of this experiment, I will endeavour to show those results which it ought to have exhibited, on the supposition that heat is propagated, even in the interior of solid bodies, by *radiations* emanating from the surfaces of the particles composing these bodies.

The particles of solid bodies are distant from each other.

On this supposition we must necessarily consider the particles that compose bodies as being *separate from each other*, and even to pretty considerable distances compared with the diameters of these particles; but there is nothing repugnant to the admission of this supposition; on the contrary, there are many phenomena which apparently indicate that all the solid bodies with which we are acquainted are thus formed.

To

To see now by what law heat would be propagated in a solid cylinder, let us represent the axis of this cylinder by a right line A E, *Plate* VII. *Fig.* 1; (see our last number) and let us begin with supposing that the cylinder consists of three particles of matter only, A C E, placed at equal distances in that line.

If the solid cylinder consist of three equi-distant particles,

Let us farther suppose, that the extremity A of the cylinder is constantly at the temperature of boiling water, while its other extremity, E, remains invariably at the freezing point.

—and the extremes at the boiling and freezing temperatures.

By an experiment, of which I have already given an account to the class*, I found that when two equal bodies, A B, one hotter than the other, are isolated and placed opposite each other, the intensities of their radiations are such, that a third body, C, placed in the middle of the space that separates them, will acquire a temperature by the simultaneous action of these radiations, which will be an arithmetical mean between those of the two bodies A and B.

Then the middle particle would have the arith. mean temperature: A body midway between two others acquires the mean heat by radiation,

From the result of this experiment we have ground to conclude, that if the cylinder were composed of three particles of matter only, A, C, E, the particle C, which is in the middle of the cylinder, must necessarily have the arithmetical mean temperature between that of A and that of E, which are at the two extremities of the cylinder; that is to say, between 212° and 32° of Fahrenheit, which is 122°.

—or 122°.

Now let us interpose between the particles A, C, and E, two other particles B D, and see whether the introduction of these two particles will make any change in the temperature of the particle C that occupies the middle of the cylinder.

Suppose two more particles to be interposed.

If the particle B be placed in the middle of the space comprised between the extremity A of the cylinder and its middle, C, it ought to acquire a mean temperature between that of the extremity A of the cylinder, and that of the point C, namely that of 167°, the mean between 212° and 122°; and if the particle D be placed in the midst of the space comprised between the middle of the cylinder and its other extremity, E, this particle ought to acquire a mean temperature between that of the middle of the cylinder and that of its extremity E; it ought then to have the temperature of 77°.

These would each take the mean temperature between the middle particle and the nearest end particle,

From this new arrangement, the particle C, situate in the middle of the cylinder, will find for its neighbours on one side the

—and therefore they would not alter the temperature,

* See our Journal, IX. 193.

the particle B, at the temperature of 167°, and on the other the particle D, at that of 77°. The point in question is, whether the presence of these two particles will make any change in the temperature of the particle C, or not.

—of that middle particle.

In the first place it is evident, that if the calorific influences of the particle B on the particle C be as efficacious in heating it, as the frigorific influences of the particle D be in cooling it, the temperature of the particle C ought not to be changed. But experience has shewn, that, at equal distances and equal intervals of temperature, the calorific influences of hot bodies, and the frigorific influences of cold bodies, are exactly equal; and as the distance from B to C is equal to the distance from D to C, while the interval of temperature between B and C = 45°, is the same as that between D and C = 45°; it is evident that the temperature of the particle C, which is in the middle of the cylinder, can be no way affected by the introduction of the intermediate particles B and D.

And by the same reason it would not be changed by other particles interposed.

By the same way of reasoning may be proved, that the introduction of an indefinite number of intermediate particles would produce no change in the temperature of the middle of the axis of the cylinder, or in any part of it; and if the introduction of an indefinite number of intermediate particles make no change in the state of a thermometer placed in the middle of the axis of the cylinder, we may conclude that the thermometer would remain equally stationary, if the number of intermediate particles were increased till they had that proximity to each other which is necessary to constitute a solid body. If, instead of a single row of particles in a right line, there were a bundle composed of an indefinite number of such rows placed side by side, forming a solid cylinder, the temperature in the different parts of the line A E would remain the same.

But the temperature of a continued solid should decrease from one particle to another in arithmetical progression.

From this reasoning we may infer, that the temperatures of the different parts of the cylinder should decrease in arithmetical progression from one extremity of the cylinder to the other.

This is true only when the solid is remote from other bodies.

But it is evident, that this law of decrement of temperature could take place only in the single case of the surface of the cylinder being completely isolated, so as to be no way affected by the action of surrounding bodies, which is absolutely impossible.

Our experiments are always thus influenced.

The circumstances under which the experiments were made are very different from those here taken for granted. The bodies

bodies we subject to experiment are constantly surrounded on all sides by the air and other bodies which act on air instruments continually, and often in a very perceptible manner; and we can never hope to isolate a cylinder so completely that the apparent progress of heat in its interior shall perceptibly obey the law we have just discovered. In common cases it deviates widely from this law.

As the causes of this deviation are well known, we will see whether there be no means of appreciating their effects. *Appreciation of the effect.*

The surface of the cylinder being surrounded by the atmospheric air and other bodies, all which are of a known and sensibly constant temperature, we may determine the comparative effects of these bodies on the different parts of the surface of the cylinder. *The atmosphere will affect the cylinder,*

In those parts of the cylinder which are hotter than the air and other surrounding bodies, the surface of the cylinder will be cooled by the action of these bodies; but if one of the extremities of the cylinder be colder than the atmospheric air, those parts of the cylinder which are colder than the circumambient fluid will be *heated* by its influence and that of the surrounding bodies. *——by cooling its hot part and heating its cold part.*

We will begin with examining the case where the coldest extremity of the cylinder is at the same temperature as the surrounding air. Let us suppose then, that the experiment with boiling water at the one end and freezing at the other be made when the temperature of the air is at the freezing point, or 32° of Fahrenheit. *If one extremity of the solid be at the temperature of the air, the other hotter,*

In this case it is evident that the surface of the cylinder must every where be *cooled* by the influence of the surrounding atmosphere. The question then is to determine the comparative effects, or the relative quantities of refrigeration or loss of heat, that must take place *in the different parts of the cylinder:* and in the first place it is clear, that the hotter a given part of the cylinder is, the more heat it must lose in a given time, by the influence of the surrounding cold bodies; whence we may conclude, that the refrigeration of the surface of the cylinder by the influence of the air and other surrounding cold bodies must necessarily diminish from the extremity of the cylinder A, which is in contact with the hot water, to its extremity E, which is in contact with the cold. *——the surface will be every where cooled, —but most so where the heat is greatest.*

From

The change is in proportion to the difference of temperature.

From reasoning which appears incontrovertible, and which the results of a great number of experiments appear to confirm, it has been concluded that the celerity with which a hot body placed in a cold medium is cooled, is always proportional to the difference between the temperature of the hot body and that of the medium. Considering this conclusion as established, we may determine *à priori* what ought to be the gradation of temperatures in the interior of a given solid cylinder surrounded by air, one extremity of which is in contact with a considerable body of boiling water, while the other is similarly in contact with cold.

We have seen that, if the surface of the cylinder were perfectly isolated, the decrease of temperature from the hottest extremity of the cylinder A to its other extremity E, which is in contact with cold water would be *in arithmetical progression*, and it has just been shewn, that the decrease must necessarily be accelerated by the action of the air and other surrounding cold bodies.

Whence the temperatures will be in geometrical progression,

But the acceleration of the decrease of temperature in those parts of the cylinder which are toward the cold extremity, depending on the action of the air and surrounding bodies, must be continually diminishing in proportion as the temperature of the surface of the cylinder approaches nearer and nearer that of the air; and hence we may conclude, that if a given number of points at equal distances from each other, be taken in the axis of the cylinder, the temperatures corresponding with these points will be *in geometrical progression*.

—and may be represented by the logarithmic curve.

We may represent the progress of the decrease of temperature by *Fig.* 2. *Pl.* VII.

Figure constructed.

In a right line A E, representing the axis of the cylinder, if we take the three points B, C and D, so that the distances A B, B C, C D, and D E shall be equal; and, erecting the perpendiculars A F, B G, C H, D I, E K, take A F = the temperature of the cylinder at its extremity A, B G = its temperature at the point B, and so of the rest; the ordinates A F, B G, &c. will be in geometrical progression, while their corresponding abscisses are in arithmetical progression; consequently the curve P Q, which touches the extremities of all these ordinates must necessarily be the *logarithmic* curve.

Comparison of the theory with the experiment.

We will now see, whether the results of experiment agree with the theory here exhibited or not.

To

To form our judgment with ease, and as it were at a single glance, of the agreement of our theory with the results of the experiment, of which I gave an account at the beginning of this memoir, we have only to represent these results by a figure in the following manner.

Construction of a figure exhibiting by a curve, and its ordinates the temperatures actually observed.

On the horizontal line AE, *Fig.* 3. representing the axis of the cylinder employed in the experiment, we will take three points, B, C and D; one, C, in the middle of the axis, being the situation of the central thermometer, the other two, B and D, at the intermediate points which the other two thermometers occupied between the middle of the axis and its two extremities.

Erecting the perpendiculars A *f*, B *g*, C *h*, D *i*, and E *k*, on the points A, B, C, D and E; and taking the ordinate A *f*= 212, the temperature of boiling water; B *g*=162, the temperature indicated by the thermometer B; C *h*=132¼, the temperature indicated by the thermometer C; D *i*=106½, the temperature given by the thermometer D; and lastly, E *k*= 32, the temperature of water mixed with pounded ice; a curve, PQ, passing through the points *f*, *g*, *h*, *i*, *k*, ought to be the *logarithmic*; that is, supposing the temperature of the surrounding air to be constantly at the temperature of melting ice during the experiment.

The curve has a point of contrary flexure.

But the experiment in question was made when the temperature of the air was at 78° F. consequently, reckoning from a certain point, taken in the length of the cylinder, where the temperature was at 78°, to the extremity E, the influence of the surrounding air, instead of *cooling* the surface of the cylinder, *heated* it; and it is evident, that the curve PQ must necessarily in this case have a point of inflexion.

It is likewise irregular.

In fact it appears on a simple inspection of the figure, that the curve PQ has a point of inflexion; but we see likewise, that this curve is not regular. That branch which is concave toward the axis of the cylinder is not similar to the adjoining portion of the curve, of equal length, which is convex toward that axis; as it ought to be according to our theory; and even the part of the curve which is convex toward the axis AE, differs sensibly from the logarithmic, particularly toward its extremity P.

The deviation is too much to be owing to the defects of thermometers.

It ought necessarily to differ from this curve, *as far as the divisions of our thermometers are defective*; but the deviation be-

tween the ordinates A *f* and B *g*, indicated by the results of the experiment in question, appears to me much too considerable to be ascribed to the imperfection of our thermometers.

It differs greatly from the logarithmic.

To see how far the curve P Q differs from the logarithmic, we have only to draw a logarithmic curve R S through the points *g* and *i*, and we shall find, that the ordinates corresponding to the points

	A,	B,	C,	D,	E.
Instead of being	212°	162°	132°¼	106½	32°
Will be -	199.55	162	131	106½	86.35
Difference -	−2·45	0	−1¼	0	+54·35

Ascribed to water being a bad conductor of heat,

The very great difference that exists between the temperature of cold water, and that indicated by the results of the experiment for the extremity of the cylinder which was in contact with this water, led me to suspect, that it was owing to the quality possessed by water in common with other fluids, which renders it *a very bad conductor of heat*.

—and the currents in the cold water being inconsiderable.

If it be true, as I believe I have elsewhere proved, that there is no sensible communication of heat between the adjacent particles of a fluid, from one to another; and that heat is propagated through fluids only in consequence of a motion of their particles, resulting from a change in their specific gravity, occasioned by their being heated or cooled: as the specific gravity of water is very little altered by an inconsiderable change of temperature when this fluid is near the freezing point, it might have been foreseen, that a solid body a little heated, and plunged into cold water, would be very slowly cooled.

Experiment to prove this.

The result of the following experiment, which I made with a view to elucidate this point, will put the fact out of a doubt.

When the cold water was briskly stirred the thermometers were all greatly depressed.

The three thermometers being stationary, one, B, at 1[illegible]°, the second, C, at 132¼°, and the third, D, at 106½ [illegible] water in contact with one of the extremities of the cylin[illegible] being still boiling, while the water mixed with pounded i[illegible] which was in contact with the other extremity, was constant[illegible] at the temperature of melting ice, I began to stir this mixtu[illegible] of ice and water pretty briskly with a little stick, and I co[illegible]tinued to stir it uninterruptedly, and with the same velocit[illegible] for two and twenty minutes.

I had scarcely begun this operation, when I had a proo[illegible] th[illegible]

that my conjectures were well founded. The mercury in the three thermometers immediately began to defcend, and did not ftop till it had fallen very confiderably.

The thermometer B fell from 162° to 152°; C from $132\frac{3}{4}$° to $111\frac{3}{4}$°; and D from $106\frac{1}{2}$° to $78\frac{1}{2}$°. Quantities of depreffion.

On comparing thefe numbers we find, that, in confequence of the agitation of the cold water for two and twenty minutes, the thermometer B fell 10° of Fahrenheit's fcale, the thermometer C 21°, and the thermometer D 28°.

As foon as I had ceafed to ftir the cold water, the three thermometers began to rife, and at the end of a quarter of an hour they had all reached the points from which they fet out at the beginning of this operation.

To facilitate the comparifon of the refults of thefe two experiments, one made with cold water at reft, the other with the fame water in a ftate of conftant agitation, I have reprefented them in *Fig.* 4. Diagram to reprefent thefe effects.

In the firft place we fhall learn feveral very interefting facts by fimple infpection of this figure; we fhall fee, 1ft. that the progrefs of refrigeration, or, to fpeak more properly, *the decreafe of temperature,* was every where much more rapid, when the cold water in contact with the extremity of the cylinder E was agitated when it was at reft. Obfervations upon this experiment.

2dly. That the extremity of the cylinder in contact with this water was conftantly near 30° colder in the firft cafe than in the fecond.

3dly. We fhall fee, that the progrefs of refrigeration was every where, and in both the experiments, fuch nearly as our theory points out.

The decreafe of temperature toward the middle of the cylinder was fo regular, that it is more than probable the apparent irregularities toward the two extremities were occafioned folely by the difficulty which a body of water finds in communicating its mean temperature to a folid, with which it is in contact.

The boiling water being in continual motion owing to its ebullition, it had a great advantage over the cold water, which was at reft, in communicating its temperature to the extremity of the cylinder it touched; but I have found, notwithftanding this, that by agitating the boiling water ftrongly with a quill, and particularly when with the quill I made a rapid Agitation increafed the effect of the boiling water likewife.

 friction

friction against the end of the cylinder immersed in the boiling water, I occasioned all the thermometers to rise several degrees.

The difference between the experiment and the theory confirm its truth;

It may perhaps be imagined, at first sight of the results of the experiment, that, as the three thermometers, which occupied the parts about the middle of the axis of the cylinder, did not indicate a decrease perfectly agreeing with the theory, the theory itself cannot be true: but a moment's reflection will show, that this inference would be too hasty, and that the difference between the theory and the results of our experiments, far from proving any thing adverse to the theory, serve on the contrary to render it more probable.

because the scales of our thermometers are defective.

The results of such experiments can never agree with the theory, except the divisions of our thermometers be perfectly accurate: but it is well known to every one, who has any knowledge of natural philosophy, that the divisions of our thermometers are defective.

To improve this instrument is an object of importance.

One of the objects I had in view in the experiments, of which I have just given an account to the class, and in several others, which I intend to make without delay, is to improve the division of the scale of the thermometer, in order to render this valuable instrument of greater utility in the delicate investigations of natural philosophy.

The air thermometer deserves to be attended to.

It appears certain, that the increase of the elasticity of air by heat is much more nearly proportionate to the increase of temperature, than the dilatation of mercury or any known fluid; consequently it is the air thermometer we ought to endeavour to improve, and which must ultimately afford us the most accurate measure of heat, that it is possible for us to procure.

SECT. IV. *The Heat produced in a Body by a given Quantity of Solar Light is the same whether the Rays be denser or rarer, convergent, parallel, or divergent.*

Whether the quantity of heat generated by the solar rays be proportional to the light absorbed.

In all cases where the rays of the sun strike on the surface of an opake body without being reflected, heat is generated, and the temperature of the body is increased: but is the quantity of heat thus excited always in proportion to the quantity of light that has disappeared? This is a very interesting question, and has not hitherto found a decisive solution.

When

When we consider the prodigious intensity of the heat excited in the focus of a burning mirror or a lens, we are tempted to believe, that the concentration and condensation of the solar rays increase their power of exciting heat; but, if we examine the matter more closely, we are obliged to confess, that such an augmentation would be inexplicable. It would be equally so on both the hypotheses, which natural philosophers have formed of the nature of light: for, as it has been proved both by calculation and experiment, that two undulations in an elastic fluid may approach and even cross each other, without deranging either their respective directions or velocities, if light be analogous to sound, we do not see how the concentration or condensation of these undulations can increase their force of impulse: and if light be a real emanation, as its velocity is not altered, either by the change of direction it undergoes in passing through a lens, or by its reflection from the surface of a polished body, it seems to me, that the power of each of these particles to excite or impart heat, must necessarily be the same after refraction or reflection as before; and consequently, that the heat communicated or excited must be, in all cases, as the quantity of light absorbed.

It does not follow from reasoning that the *absolute* power of the rays to produce heat can be increased by condensing them.

I have just made some experiments, which appear to me to establish this fact beyond question.

Experimental investigation.

Having procured from the optician Lerebours two lenses perfectly equal, and of the same kind of glass, four inches in diameter, and of eleven and a half focus, I exposed them at the same time to the sun, side by side, about noon, when the sky was very clear; and by means of two thermometers, or reservoirs of heat, of a peculiar construction, I determined the relative quantities of heat, that were excited in given times by the solar rays at different distances from the foci of the lenses.

Two convex lenses perfectly similar were used,

The two reservoirs of heat are a sort of flat boxes of brass filled with water. Each of these reservoirs is three inches ten lines and a half in diameter, and six lines thick, well polished externally on all sides except one of its two flat faces, which was blackened by the smoke of a candle. On this face the solar rays were received in the experiments.

to throw the sun's light upon flat tin boxes containing water and blackened on their surfaces.

Each of these reservoirs of heat weighs when empty 6810 grains, *poids de marc*, (near a pound troy); and contains 1210 grains of water (about 2 oz. 2 dwts.

Taking

Taking the capacity of brafs for heat to be to that of water as 0,11 to 1, it appears, that the capacity of the metallic box, weighing 6850 grains, is equal to the capacity of 622 grains of water; and adding this quantity of water to that contained in the box, we fhall have the capacity of the refervoir prepared for the experiments equal to that of 1932 grains of water.

The temperature of the water in each was fhewn by a thermometer.

Each refervoir is kept in its place by a cylinder of dry wood, one of the extremities of the cylinder being fixed in a focket in the center of the interior face of the refervoir; and each refervoir has a little neck, through which it is filled with water, and which after receives the bulb of a cylindrical thermometer, that reaches completely acrofs the infide of the box in the direction of its diameter.

The two refervoirs of heat, with their two lenfes, are firmly fixed in an open frame, which being moveable in all directions by means of a pivot and a hinge, the apparatus is eafily directed toward the fun, and made to follow its motion regularly, fo as to keep the folar fpectra conftantly in the centers of the blackened faces of the refervoirs.

Light admitted through equal apertures.

In order that the quantities of light paffing through the two lenfes fhould be perfectly equal, a circular plate of well polifhed brafs, in the centre of which is a circular hole three inches and a half in diameter, is placed immediately before each of the lenfes.

When the refervoirs of heat are placed at different diftances from the focufes of their refpective lenfes, the diameters of the folar fpectra, which are formed on the blackened faces of the refervoirs, are neceffarily different; and as the quantities of light are equal, its denfity at the furface of each refervoir is inverfely as the fquare of the diameter of the fpectrum formed on that furface.

Experiment I.

Experiment. With equal apertures the folar fpots from the lenfes were of 6 and of 24 lines diam.

In this experiment the refervoir A was placed fo near the focus of the lens, between the lens and the focus, that the diameter of the folar fpectrum falling on it was only ½ an inch, or 6 lines, while the refervoir B was advanced fo far before the focus, that the fpectrum was two inches in diameter, or 24 lines.

As

As the quantities of light falling on both were equal, the density of the light at the surface of the reservoir A was to the density of that at the surface of the reservoir B, as the square of 24 to the square of 6, or as 16 to 1.

The densities of these equal quantities of light were therefore as 16 to 1;

I imagined, that, if the quantity of heat, which a given quantity of light is capable of exciting, depended any way on its density, as the densities were so different in this experiment, I could not fail to discover the fact by the difference of time, which it would require to raise the two thermometers the same number of degrees.

Having continued the experiment more than an hour, on a very fine day, when the sun was near the meridian and shone extremely bright, I did not find, that one of the reservoirs was heated perceptibly quicker than the other.

but both the vessels were heated in equal times.

Experiment II.

I placed the reservoir of heat A still nearer the focus of the lens, in a situation where the solar spectrum was only $4\frac{1}{4}$ lines in diameter, and where blackened paper caught fire in two or three seconds; and I removed the reservoir B still farther from the focus, advancing it forward till the diameter of the spectrum was two inches three lines.

Experiment wherein the diameters of the spots were as $4\frac{1}{4}$ to 27.

The densities of the light at the surfaces of the reservoirs in this experiment were as 32 to 1.

The densities of the light were as 32 to 1.

The temperature of the reservoirs, as well as that of the atmosphere, at the beginning of the experiment, was 54° F. $= 9^{\circ}\ \frac{7}{9}$ R.

The reservoir A, after having been exposed to the action of very intense light near the focus of the lens for twenty-four minutes forty seconds, was raised to the temperature of 80° F. $= 21^{\circ}\ \frac{1}{9}$ R.

The densest light afforded rather less heat.

The reservoir B, which was much farther from the focus of its lens, was raised to the same temperature, 80° F. a little more quickly, or in twenty-three minutes forty seconds.

To raise the temperature of the reservoir A to 100° F. $= 30^{\circ}\ \frac{2}{9}$ R. it was necessary to continue the experiment for one hour fifteen minutes ten seconds, reckoning from the commencement of it; but the reservoir B reached the same temperature in one hour twelve minutes ten seconds.

The progress of this experiment from the beginning to the end is exhibited in the following table:

Increases

The general results tabulated.

Increases of Temperature.	Time taken By A.	Time taken By B.
From 54° to 80° F.	24′ 40″	23′ 40″
80 85	7 45	7 30
85 90	9 55	9 0
90 95	13 30	13 0
95 100	19 20	19 0
54 100	75 10	72 10

Time of the experiment.

This experiment was begun at 7 minutes 30 seconds after 11, and finished at 22 minutes 40 seconds after 12, the sky being perfectly clear during the time.

Hence light does not give more heat absolutely by being condensed.

On comparing all the results of this experiment, we see, that the reservoir A, which was placed very near the focus, was more slowly heated than the reservoir B, which was at a considerable distance from it *. The differences of time however taken to heat them an equal number of degrees were very trifling, and I think may be easily explained, without supposing the condensation of light to increase (*qu.* diminish?) its faculty of exciting heat.

The rays were convergent in the preceding experiments.

In both the preceding experiments the solar rays striking on the reservoirs of heat were *convergent*, and they were even equally so on both sides. To determine whether *parallel* rays have the same power of exciting heat as convergent rays, I made the following experiment.

Experiment III.

When one vessel was exposed to the parallel rays of the sun without interception,

Having removed the lens from before the reservoir B, I suffered the direct rays of the sun to fall on the blackened face of the reservoir, through the circular hole three inches and half in diameter in the round brass plate, which had been constantly placed before that lens in the preceding experiments.

The reservoir A was placed behind its lens as in the former experiments, and at the place where the solar spectrum had six lines diameter.

* Did not the elevated temperature of the smaller surface sustain its power of absorbing heat, conformably to the known laws of heated bodies?—N.

Having

Having exposed this apparatus to the sun, I found, that the reservoir B, on which the direct rays fell, was heated sensibly quicker than the reservoir A, which was exposed to the action of the concentrated rays near the focus of the lens.

It was heated more quickly than the other by a spectrum of 6 lines or one-seventh part diam.

The temperature of the apparatus and of the atmosphere at the beginning of the experiment being 53° F. = 9° $\frac{1}{3}$ R. the reservoir A required twenty-three minutes thirty seconds to raise it to the temperature of 80° F. = 21° $\frac{1}{3}$ R; but the reservoir B, which was exposed to the direct rays of the sun, acquired the same temperature in eighteen minutes thirty seconds.

To reach the temperature of 100° F. = 30° $\frac{2}{9}$ R. took the reservoir A one hour and three minutes, but the reservoir B forty-seven minutes fifteen seconds only.

The following table will show the progress of this experiment from the beginning to the end.

General results.

Increases of Temperature.		Time taken By A.		Time taken By B.	
From 53°	to 65° F.	8′	26″	7′	0″
65	70	4	10	3	15
70	75	5	10	3	45
75	80	5	40	4	30
80	85	7	0	4	45
85	90	7	30	5	45
90	95	10	30	8	0
95	100	13	10	10	15
100	105	20	0	14	45
53	105	81	36	62	30

As a considerable part of the light that fell on the lens before the reservoir A, was lost in passing through it, it is evident, that the quantity received by this reservoir was less than that received by the reservoir B, which was exposed to the direct rays of the sun; and we have seen, that the latter was heated more rapidly than the former.

This difference ascribed to light being lost in passing through the lens.

As we know not exactly how much light was lost in passing through the lens, we cannot determine from the results of this experiment, whether convergent rays be more or less efficacious in exciting heat than parallel rays; but the difference in the times

This experiment is not decisive: but

times of heating was not greater, as it appears to me, than we might have expected to find it, supposing it to be occasioned solely by the difference between the quantities of light acting on the reservoirs.

The result of the following experiment will establish this point beyond doubt.

Experiment IV.

Exp. 4 was made with equal apertures and spectra; but the one being *within* the focus was formed by convergent rays, and the other *without* by divergent rays.

Having replaced the lens belonging to the reservoir B, I adjusted this reservoir to such a distance between the lens and its focus, that the solar spectrum was one inch in diameter; and I placed the reservoir A at the same distance beyond its focus.

As the quantities of light directed toward both were equal; and the diameters of the spectra, consequently the densities of the light that formed them, were also equal; there could be no difference between the results of the experiments with the two reservoirs, except what was occasioned by the difference in the *direction* of the rays that formed the spectra. On one hand these rays were *convergent*, and on the other *divergent*; and I had inferred, that if parallel rays were in reality less efficacious in exciting heat than convergent rays, as some philosophers have supposed, *divergent* rays must be still less efficacious than parallel rays, and consequently much less than convergent rays.

No sensible difference occurred.

Having made the experiment with all possible care, I found no sensible difference between the quantities of heat excited in a given time by divergent and convergent rays.

The following are the particulars of the progress and results of this experiment:

General results of this last experiment.

Increases of Heat.		Time taken	
		By A, with divergent Rays.	By B, with convergent Rays.
From 60°	to 65° F.	4′ 50″	4′ 50″
65	70	4 55	5 0
70	75	5 27	5 25
75	80	6 13	6 15
60	80	21 25	21 30

From

From the results of all the experiments, of which I have just given an account to the class, we may conclude, that the quantity of heat excited or communicated by the solar rays is always, and under all circumstances, as the quantity of light that disappears.

Conclusion. The quantity of heat is always as the light absorbed.

III.

Observations on blasting Rocks; with an Account of an Improvement, whereby the Danger of accidental Explosion is in a great Measure obviated. By Mr. William Close. *From the Author.*

To Mr. NICHOLSON.

SIR, *Dalton, Oct.* 14, 1805.

THE method of confining the force of gunpowder by a column of sand in blasting rocks, has been several years used in this part of Furness: At one time it was a very favourite practice; but at present, from the prejudices or indifference of workmen, or on account of the little danger attendant on working lime-stone in the common manner, it is less in repute.

Practice of blasting with sand in Furness,

About two years ago, supposing this method not to be generally known, I drew up a short account of it, and should have sent it to the Philosophical Journal, had it not been connected with other miscellaneous matter, which I had given to Mr. G. Ashburner, the printer and proprietor of a new edition of West's Antiquities of Furness, in which work the process is described and recommended *.

noticed by the author elsewhere.

Though

* The passage alluded to is as follows, p. 393. "In breaking up the loose rocks upon Bayclift Haggs, after the enclosure of that common, a method of employing sea-sand, for the purpose of confining the force of gunpowder in blasting, was used, which does not appear to be generally known, though it was undoubtedly in use in other parts before it was adopted in Furness. The method is briefly this: After the excavation is made in the usual manner with a borer, the charge of powder is poured in, and a priming-straw of a proper length, filled with powder, is placed in the hole, having one of its sides near the lower end so cut or thinned, that the

Improvement by Mr. Fisher announced.

Though this method is undoubtedly worthy of much attention, and may often be employed with advantage; yet, when a strong charge is required, the common mode of stemming must be frequently adopted. And as the danger in blasting some kinds of rocks in this manner is very considerable, I am happy to notice an easy method of obviating one principal cause of accidental explosion, which was communicated to me in conversation, a few days ago, by Mr. Thomas Fisher, a respectable slate merchant in this town, who assures me it is infallible.

Causes of accidental explosion. The principal is from the friction of the iron pricker in drawing.

The principal danger attendant on blasting, does not consist in stemming upon the charge of powder, but in the subsequent operation of drawing the iron rod, called the pricker, which makes the channel for the priming-straw. For although the collision of the first fragments of stemming sometimes produces an explosion, yet this may be prevented by previously ramming a thick cap of paper, &c. upon the powder; by beating lightly upon the first pieces of stone that are thrown into the hole; or by using those materials for stemming which are least liable to give fire, such as rotten stone, pieces of broken pots, or burnt clay. The pricker being hard pressed against the rock, and in close contact with the stemming, cannot be drawn out by hand, but must be struck out by the hammer, a strong piece of iron called a jumper being first placed in an eye or loop in the highest part of the rod, to receive the blows which are given in a proper direction to bring it out of its place. Now it frequently happens, that the friction of the lowest part of the pricker against the rock fires the powder at the first or second blow. When the explosion happens at the commencement of stemming, the workman generally sustains only a partial injury; but when in this part of the operation, when the powder exerts its whole force, and

the charge may partially communicate with the small ascending column contained in the straw. After this, the remainder of the excavation is filled, by pouring in dry sea-sand; and the explosion is given, by firing the priming-straw in any of the various ways which are in common use.

"This method has been found to be equally effectual as stemming with any of the common materials; and where it can be used is certainly preferable: it is safer, simpler, and more expeditious."

disperses

dispersed pieces of the shattered rock in various directions, his life is in the utmost danger, and his situation is truly terrible to contemplate.

Mr. Fisher's improvement is to obviate this danger; and consists in the use of a copper rod, or pricker, for making the hole that receives the priming-straw, instead of one of iron, which before was every where employed in this part of the kingdom.

Mr. F.'s expedient is a pricker made of copper, which is not liable to fire the powder.

In our conversation Mr. F. observed, that some years ago, three explosions happened on drawing the pricker, in the course of a fortnight, at his quarry in Kirkby Ireleth, and that one man being killed and two wounded, several of the workmen were so intimidated, that they resolved to abandon a place which they considered as destined to daily misfortunes. It therefore became highly requisite, on several accounts, to attempt some innovation for the security and encouragement of the workmen.

In meditating on the cause of these accidents, it appeared most rational to attribute them to the iron pricker giving fire by its friction against the rock, which was a hard blue rag, or whinstone; and from this view of the cause it was inferred, that safety would accrue from the use of prickers constructed of those metals which are least disposed to give fire with stone. Mr. Fisher, therefore, determined to make trial of copper, and having procured some implements of this kind, found them to answer the purpose completely. It is now upwards of three years since this improvement was adopted, and as no explosion has happened at the end of stemming in that period, at an extensive work where accidents were frequent before, Mr. F. considers the means as almost infallible; and is happy to think that many sad misfortunes have been thereby prevented.

Ample experience has proved its utility.

There are eleven slate quarries in Kirkby Ireleth, at several of which copper rods are now used; but at others they are not. At one of these a fatal accident happened a few months ago, from an explosion upon drawing a rod of iron.

Prickers, such as used by Mr. Fisher, are easily constructed: A piece of copper being forged to the proper length, shape, and thickness for the body of the tool, is rivetted to an iron head or loop similar to that of the common pricker. These implements, when carefully used, are nearly as durable as those of iron.

5 Sand

Advantage of strong charges in firm rock.

Sand has not hitherto been used in blasting at the slate quarries in Kirkby Ireleth. The masters do not think it would succeed well in their work. I have frequently seen Mr. Fisher use it in limestone rock near this town: He says it answers the best in deep holes, but thinks that sand is more liable to be blown out than stemming. He also considers it as the most advantageous method of working, in driving levels, and blasting in firm rock, to use strong charges of powder, that the stone may be sufficiently broken by the explosion to be removed without much assistance from the hammer, the pick, or the lever: For thus the expedition of the work amply compensates for the small addition which is requisite to a common charge of powder.

I am, Sir,

Your's respectfully,

WILLIAM CLOSE.

IV.

Description of a portable Steam-engine, invented by Mr. SAMUEL CLEGG *, *David Street, Manchester. Communicated by Mr.* DALTON, *Lecturer at the Royal Institution, &c.*

Description of a Steam-engine.

THIS engine is worked by four copper valves in the usual manner, but the mechanism for lifting them is very different from any hitherto made: there are no levers employed for opening the valves, and there is no hand gear. The steam and exhaustion valves are on the same horizontal plane; those which are vertical to each other are not like those hitherto used, both exposed to the steam or both to a vacuum, but by a simple contrivance in the construction of the nozzles, the one is exposed to the steam while the other has a communication with the condensing vessel. From what has been said it may easily be perceived, if the two valves be connected together by a straight rod, that when this rod is lifted, the pressure is given to the piston, and the machine is put into motion; and if the other two valves be connected in the same manner

* Late apprentice to Messrs. Boulton and Watts, of Birmingham.

manner and lifted at an appointed time, the engine is kept in motion. The outfide appearance of thefe nozzles may be feen at *Fig*. 1, *c c*, (*Plate* IX.) The rods which come out of the bottom of the nozzles are kept tight by vertical ftuffing-boxes, the whole of which is hid in the drawing by the frame.

Defcription of a fteam-engine.

The next is a new contrivance for producing a rotative motion from a reciprocating one, which not only fimplifies the machine very much, but exceeds the power of the common crank by nearly one-third, in confequence of its acting always perpendicular to the radius of the wheel, which is done by a rack and wheel, as reprefented by *Fig*. 2 and 3; and as this plan of connection diftributes the power uniformly, of courfe a much lighter fly-wheel is required, which diminifhes friction, &c.

Explanation of the Plate.

Fig. 1. Is a reprefentation of the engine: one of the corner columns A A, which fupports the frame, ferves likewife for an eduction-pipe and condenfing-veffel: the air-pump E is joined to the condenfing veffel by the pipe D; *e* is the pifton-rod, and though it works out at the bottom of the cylinder, it is as eafily kept tight as if it worked out at the top; *b* is a fimilar rod which keeps the rack perpendicular; *a a* are the two radius bars on which the braffes are fixed that fupport the fhaft; by this contrivance the wheel C eafily moves from one fide to the other of the rack F.

Fig. 2. is a view of the rack on a larger fcale, where C reprefents the wheel and D the fhaft; E E, a fliding-bar, on which is fixed the fmall roller *o*, ferving as a connecting link to keep the wheel C always in gear; for, when the wheel is in gear on the oppofite fide of the rack, the roller *o* is on the other fide of the plate *a a*; but it will perhaps be more clearly underftood by the plan, *Fig*. 3. where the letters reprefent the fame movement as in the elevation, *Fig*. 2. This defcription may be eafily underftood by thofe who already poffefs a little knowledge of a fteam-engine.

Manchefter, *Oct*. 5, 1805.

Letter

V.

Letter from Mr. J. C. HORNBLOWER, *describing the framed Work by which the Roof of Clapham Church was raised to its original Situation, without disturbing the Interior of the Building, &c.*

To Mr. NICHOLSON.

DEAR SIR,

Framed truss by which the roof of Clapham church was raised, &c.

IT will be a pleasure to you I know to record the productions of genius or fancy in your valuable Work, and therefore I have no hesitation in presenting the inclosed for that purpose.

It is the invention of Mr. Watkin Bloore, one of the partners of Fothergal and Co. carpenters at Clapham, and was invented to raise the sunk roof of Clapham church; and its application to the purpose intended, exhibits at once the means and the end that was to be accomplished; as by it, the roof was *raised* and *secured*, in the same process, without incommoding the building with floors and scaffolds, which must have occasioned considerable damage to the furniture of the church.

The shaded part of the drawing, *Plate* X, shews the truss, and the lines behind it the construction of the roof. The middle piece in the truss marked A, is joggled into the king-post of the roof, and the two screws put into action raise it up, and with it the whole of the middle or sunk part of the roof, all which is easily comprehended by the drawing.

The drawing, *Fig.* 3, shews an improved mode of constructing the truss, by the riders A A A A being framed over the principals B B, by which the raising screws are more firmly supported in elevating the queen-posts C C in the roof.

This must be a valuable experiment in the art of carpentry, which, considering how little science of it falls to the lot of its possessors, cannot be too much regarded.

I am, Dear Sir,

Your very obedient servant,

J. C. HORNBLOWER.

Remarks

VI.

Experiments on draining Land, by JOHN CHRISTIAN CURWEN, *Esq. M. P. of Workington-Hall, in Cumberland, with an Engraving* *.

DEAR SIR,

MUCH having been said, in the public Papers, relative to draining, on the improved method of Mr. Elkington, I beg leave to offer you some observations respecting it, which have fallen under my notice, and which tend to prove it can be applied, with success only, in such parts of the kingdom, as have few, if any, interruption of the strata. In order to make myself intelligible, it may not be improper to explain what is meant by interruptions of the strata, or dykes and fissures, as they are denominated in mining countries. They are produced by the fracture or disunion of the strata, and consist most commonly of the broken fragments of each superior strata; and towards the surface are of sand, gravel, and stones, which seldom or never fail of affording considerable quantities of water. These dykes may be approached within a few feet, and afford no water, as will be seen in two instances in the plan sent you. No. 3 is a main drain, four feet deep, which passed within a few yards of A, an extreme wet place, and did not affect it. The person employed, supposed the water to be below him, and brought in a lower level No. 1, which likewise failed. No. 2 was then made still lower, but with no better success than No. 3, though with more advantage of level. As soon as it crossed the dyke, I C, but before the level was brought up, not being deeper than the main drain, it got a considerable feeder. This proved that an interruption in the strata prevented the water flowing into a drain, which was of a depth otherwise to have drawn it. Another example occurs in the same field, at letter B; which is a sunk fence, four feet below the surface of the adjoining field, which was extremely wet within a few yards of the sunk fence.

Mr. Elkington's method of draining is applicable only where the strata are little interrupted.

Dykes and fissures.

These interruptions prevent the draining off of the water.

* From the twenty-second volume of the Transactions of the Society of Arts; who awarded the gold medal to the author. The plan he refers to is at their house.

Experiments of drainage.

fence. A lower level was supposed necessary to drain this water, and it was obtained at the dotted line. No water of any consequence was got, till it was within a few yards of the sunk fence, when a prodigious feeder was cut, and the head of the drain was not so deep at the time as the sunk fence. Many instances to the same effect might be produced. In sinking shafts in places much troubled with water, it is endeavoured, if circumstances will permit, to get near a dyke, which serves as a barrier to the water; and if, in sinking, the dyke be not crossed, the water is kept clear off; but if otherwise, the water would be got at any depth, though not in such quantities as when near the surface. The spot of ground, to which I have alluded, has above a dozen dykes, which may be traced from the out-bursts of water. They run in a direction of south to north-west. I have made my drains east and west. In one or two places, I was obliged to run a drain south. This proceeds from an arm running from the dyke: but this seldom extends to any distance, and they gradually decrease till they end; and they rather make an interruption than a breakage of the strata, as the strata is the same on each side of it. In such a country, Mr. Elkington could draw no more water than what lay in the uninterrupted strata between any two of these dykes. The method of making the drain is explained by the engraving. I had twenty years ago drained this ground with stone drains, from 20 inches to two feet; but their direction having been mostly from north to south, and not sufficiently deep, I had got little more than the day water. The feeder which I have now got, might be made applicable to many purposes. The drains are from two feet to nearly five feet deep. I have made 6000 yards in the last twelve months; the cutting from 14d. to 18d. per rod, filling 8d. ten and a half single cart-loads of stones, at 9d. each, making the cost 10s. per rod. The expence appears great; but fewer drains are required, and the work is effectually done. By reference to the plan, it will be seen that the direction of the drains not being able to draw the upper water, I was obliged to extend them. I would advise beginning at the highest level; for frequently that clears the whole, unless some dykes intervene in a contrary direction. I believe that all springs and out-bursts of water proceed from dykes. The extent of these is various. Some may be traced for many miles, and their effects seen

The drains must be made to cross the dykes.

Description of the drains by reference to the drawing.

Springs of water proceed from dykes.

feen from the water that appears on the furface. Their origin is fcarcely perceptible, and they thicken to many yards as they are approached. The ftrata on both fides have a more rapid rife or dip, and are of a clofer and harder texture. If thefe obfervations appear to you worthy of attention, you may make what ufe you think proper of them. I by no means wifh to detract from Mr. Elkington's merit; but it is not generally applicable; and in counties where the ftrata are much broken, Mr. Elkington's plan will be found to fail.

I am, Dear Sir,

Your obedient fervant,

J. C. CURWEN.

Feb. 3, 1804.

Mr. Charles Taylor.

P. S. The higheft drain is 120 feet above the level of No. 3.

A certificate from Mr. William Hoodlefs, farming agent, accompanied this letter, ftating that upwards of fix thoufand yards of drains had been cut, and completely filled, on the farm of John Chriftian Curwen, Efq.; that the firft drains made according to that plan were done three years ago; and that they ftand completely, and anfwer an admirable purpofe.

Reference to Fig. 5, Plate XI. *of the Manner in which Mr.* CURWEN'S *Drains are made.*

The loweft part of the drain below E E is twelve inches wide.

E E 4 4 are the two fide-ftones of the drain, nearly four inches thick and nine inches high.

F 9 is the aperture for the water, nine inches high.

D, the flag or thin ftone over the aperture, and which covers the fide-ftones of the drain.

C C, the body of the drain, filled with loofe ftones till within nine inches of the furface.

B 9, the top of the drain, twenty-two inches wide and nine inches deep, filled with grafs fod and foil.

VII.

Remarks on a Letter of Mr. DALTON, *concerning the Maximum Density of Water; with an Account of two Experiments of Dr.* HOPE, *tending to shew that it takes place at a Temperature above the freezing Point. In a Letter from* T. I. B.

To Mr. NICHOLSON.

SIR,

Reference to Mr. Dalton's letter.

IN No. 45 of your Journal, page 28, Mr. Dalton has published some remarks upon Count Rumford's experiments, relating to the maximum density of water, where he explains the rising of the thermometer in the cup, by observing that it acquired heat by the proper conducting power of water. This, I should think, is by no means probable; for the conducting power of water is not sufficient to produce such a rapid effect.

The circumstances of the two thermometers by the side of the ball and cup, in the Count's two first experiments, I think are perfectly consistent with his principle; the cup, very probably, did overflow, which might have been ascertained by a thermometer placed below.

In the conclusion of this letter Mr. Dalton expresses a wish that Count R. or some one in possession of a similar apparatus, would repeat the Count's first experiment, with this difference, that the mass of water should be at 40° and the ball at 32°, in which case, he says, the thermometer would not be at all affected on the Count's principle; neither would it be affected (on Mr. D.'s principle) if the tenacity of the water counteracts the force of descent: and what conclusion could be drawn from such a variation of the experiment?

Experiments of Dr. Hope.

The following experiment was made by Dr. Hope, professor of chemistry in Edinburgh, to ascertain the point at which water has the greatest density,—and it appears to me to be perfectly decisive.

He filled a jar with ice-cold water, and exposed it to the air of a room at 52°: he suspended in it two very delicate thermometers, one at half an inch from the bottom, and the other at the same distance from the surface of the water: the thermometer nearest the bottom was first affected, and con-

tinued to rise till it reached the temperature of 40°, when it became stationary: the thermometer at the surface rose more slowly, but did not stop till it acquired the temperature of the room (52°).

Dr. Hope, to render this point still clearer, exposed water at 52° to the air of a room 32°; the result corresponded perfectly with the former experiment.

If you see no objection to the publication of this letter, by inserting it in your Journal, you will oblige *,

Sir, your's,

T. I. B.

Edinburgh, Oct. 10, 1805.

VIII.

Observations and Conjectures relative to the supposed Welch Indians in the western Parts of North America. Republished from the "Kentucky Palladium," with additional Remarks and Conjectures, by the Editor of the Philadelphia Medical and Physical Journal †.

SIR,

Traditional account of inhabitants of America supposed to have originated from Wales.

No circumstance relating to the history of the Western Country, probably has excited, at different times, more general attention and anxious curiosity than the opinion, that a nation of white men, speaking the Welch language, reside high up on the Missouri. By some the idea is treated as nothing but the suggestion of bold imposture and easy credulity; whilst others regard it as a fact fully authenticated by Indian testimony and the report of various travellers worthy of credit.

The fact is accounted for, they say, by recurring to a passage in the history of Great Britain, which relates, that several years after the discovery of America by Christopher Columbus, a certain Welch prince embarked from his native country with a large party of emigrants; that after some time, a vessel

* A fuller account of the late experiments of Dr. Hope will be inserted when the Edinburgh Transactions appear.

† Extracted from that Work, Vol. II. Part I.

or

Traditional account of inhabitants of America supposed to have originated from Wales.

or two came back with the account that they had discovered a country far to the westward, and that they set sail again with a fresh reinforcement, and never returned again any more.

The country which these adventurers discovered, it has been supposed, was the continent of North America; and it has been conjectured that they landed on the continent, somewhere in the Gulf of Mexico, and from thence proceeded northward, till they got out of the reach of the hostile natives, and seated themselves in the upper country of Missouri.

Many accounts accordingly have been published, within the last thirty years, of persons who, in consequence either by accident or the ardour of curiosity, have made themselves acquainted with a nation of men on the Missouri, possessing the complexion of Europeans and the language of Welchmen.

Could the fact be well established, it would afford perhaps the most satisfactory solution of the difficulty occasioned by a view of the various ancient fortifications with which the Ohio country abounds, of any that has ever been offered. Those fortifications were evidently never made by the Indians. The Indian art of war presents nothing of the kind. The probability too is that the persons who constructed them were, *at that time,* acquainted with the use of iron: The situation of these fortifications, which are uniformly in the most fertile land of the country, indicates, that those who made them were an agricultural people; and the remarkable care and skill with which they were executed, affords traits of the genius of a people, who relied more on their military skill than on their numbers. The growth of the trees upon them is very compatible with the idea that it is not more than three hundred years ago that they were abandoned.

These hints however are thrown out rather to excite enquiry, than by way of advancing any decided opinion on the subject. Having never met with any of the persons who had seen these white Americans, nor even received their testimony near the source, I have always entertained considerable doubts about the fact. Last evening, however, Mr. John Childs, of Jessamine County, a gentleman with whom I have been long acquainted, and who is well known to be a man of veracity, communicated a relation to me, which at all events appears to merit serious attention.

After

After he had related it in converſation, I requeſted him to repeat it, and committed it to writing. It has certainly ſome internal marks of authenticity. The country which is deſcribed was altogether unknown in Virginia when the relation was given, and probably very little known to the Shawnees Indians. Yet the account of it agrees very remarkably with later diſcoveries. On the other hand, the ſtory of the large animal, though by no means incredible, has ſomething of the air of fable, and it does not ſatisfactorily appear how the long period which the party were abſent was ſpent; though Indians are, however, ſo much accuſtomed to loiter away their time, that many weeks, and even months, may probably have been ſpent in indolent repoſe.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

Without detaining you any more with preliminary remarks, I will proceed to the narration, as I received it from Mr. Childs.

Maurice Griffith, a native of Wales, which country he left when he was about ſixteen years of age, was taken a priſoner by a party of Shawnees Indians, about forty years ago, near Voſſes Fort, on the head of Roanoke river in Virginia, and carried to the Shawnees nation. Having ſtaid there about two years and a half, he found that five young men of the tribe had a deſire of attempting to explore the ſources of the Miſſouri. He prevailed upon them to admit him as one of the party. They ſet out with ſix good rifles and with ſix pounds of powder a-piece, of which they were, of courſe, very careful.

On reaching the mouth of the Miſſouri, they were ſtruck with the extraordinary appearance occaſioned by the intermixture of the muddy waters of the Miſſouri and the clear tranſparent element of the Miſſiſſippi. They ſtaid two or three days amuſing themſelves with the view of this novel ſight: they then determined on the courſe which they ſhould purſue, which happened to be ſo nearly in the courſe of the river, that they frequently came within ſight of it as they proceeded on their journey.

After travelling about thirty days through pretty farming wood land, they came into fine open prairies, on which nothing grew but long luxuriant graſs. There was a ſucceſſion of theſe varying in ſize, ſome being eight or ten miles acroſs but one of them ſo long that it occupied three days to travel through

Traditional account of inhabitants of America supposed to have originated from Wales.

through it. In passing through this large prairie, they were much distressed for water and provisions, for they saw neither beast nor bird; and, though there was an abundance of salt springs, fresh water was very scarce. In one of these prairies the salt springs ran into small ponds, in which, as the weather was hot, the water had sunk and left the edges of the ponds so covered with salt, that they fully supplied themselves with that article, and might easily have collected bushels of it. As they were travelling through the prairies they had likewise the good fortune to kill an animal, which was nine or ten feet high, and a bulk proportioned to its heighth. They had seen two of the same species before, and they saw four of them afterwards. They were swift-footed, and they had neither tusks nor horns. After having passed through the long prairie, they made it a rule never to enter on one which they could not see across, till they had supplied themselves with a sufficiency of jerked venison to last several days.

After having travelled a considerable time through the prairies, they came to very extensive lead mines, where they melted the ore, and furnished themselves with what lead they wanted. They afterwards came to two copper mines, one of which was three miles through; and in several places they met with rocks of copper ore as large as houses.

When about fifteen days journey from the second copper-mine, they came in sight of white mountains, which, though it was in the heat of summer, appeared to them to be covered with snow. The sight naturally excited considerable astonishment; but, on their approaching the mountains, they discovered that, instead of snow, they were covered with immense bodies of white sand.

They had in the mean time passed through about ten nations of Indians, from whom they received very friendly treatment. It was the practice of the party to exercise the office of spokesman in rotation; and when the language of any nation through which they passed was unknown to them, it was the duty of the spokesman, a duty in which the others never interfered, to convey their meaning by appropriate signs.

The labour of travelling through the deep sands of the mountains was excessive; but at length they relieved themselves of this difficulty, by following the course of a shallow river, the bottom of which being level, they made their way to the top of the mountains with tolerable convenience.

After

After paſſing the mountains they entered a fine fertile tract of land, which having travelled through for ſeveral days, they accidentally met with three white men in the Indian dreſs. Griffith immediately underſtood their language, as it was pure Welch, though they occaſionally made uſe of a few words with which he was not acquainted. However, as it happened to be the turn of one of his Shawnees companions to act as ſpokeſman or interpreter, he preſerved a profound ſilence, and never gave them any intimation that he underſtood the language of their new companions.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

After proceeding with them four or five days journey, they came to the village of theſe white men, where they found that the whole nation was of the ſame colour, having all the European complexion. The three men took them through their villages for about the ſpace of fifteen miles, when they came to the council-houſe, at which an aſſembly of the king and chief men of the nation was immediately held. The council laſted three days, and as the ſtrangers were not ſuppoſed to be acquainted with their language, they were ſuffered to be preſent at their deliberations.

The great queſtion before the council was, what conduct ſhould be obſerved towards the ſtrangers. From their fire-arms, their knives, and their tomahawks, it was concluded that they were a warlike people. It was conceived, that they were ſent to look out for a country for their nation; that if they were ſuffered to return, they might expect a body of powerful invaders; but that if theſe ſix men were put to death, nothing would be known of their country, and they would ſtill enjoy their poſſeſſions in ſecurity. It was finally determined that they ſhould be put to death.

Griffith then thought it was time for him to ſpeak. He addreſſed the council in the Welſh language. He informed them, that they had not been ſent by any nation; that they were actuated merely by private curioſity, they had no hoſtile intentions; that it was their wiſh to trace the Miſſouri to its ſource; and that they ſhould return to their country ſatisfied with the diſcoveries they had made, without any wiſh to diſturb the repoſe of their new acquaintances.

An inſtant aſtoniſhment glowed in the countenances, not only of the council, but of his Shawnees companions, who clearly ſaw that he was underſtood by the people of the country. Full confidence

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confidence was at once given to his declarations; the king advanced and gave him his hand. They abandoned the design of putting him and his companions to death, and from that moment treated him with the utmost friendship. Griffith and the Shawnees continued eight months in the nation; but were deterred from prosecuting their researches up the Missouri by the advice of the people of the country, who informed them, that they had gone a twelve month's journey up the river, but found it as large there as it was in their own country.

As to the history of this people, he could learn nothing satisfactory. The only account they could give was, that their forefathers had come up the river from a very distant country. They had no books, no records, no writings. They intermixed with no other people by marriage, there was not a dark-skinned man in the nation. Their numbers were very considerable. There was a continued range of settlements on the river, for fifty miles, and there were within this space three large water-courses which fell into the Missouri, on the banks of each of which they were likewise settled. He supposed that there must be fifty thousand men in the nation capable of bearing arms. Their cloathing was skins well dressed. Their houses were made of upright posts and the barks of trees. The only implement they had to cut them with, were stone tomahawks; they had no iron. Their arms were bows and arrows. They had some silver which had been hammered with stones into coarse ornaments, but it did not appear to be pure. They had neither horses, cattle, sheep, hogs, nor any domestic nor tame animals. They lived by hunting. He said nothing about their religion.

Griffith and his companions had some large iron tomahawks with them. With these they cut down a tree and prepared a canoe to return home in: But their tomahawks were so great a curiosity, and the people of the courtry were so eager to handle them, that their canoe was completed with very little labour. When this work was accomplished, they proposed to leave their new friends: Griffith, however, having promised to visit them again.

They descended the river with considerable speed, but amidst frequent dangers, from the rapidity of the current particularly when passing through the white mountains. When they reached the

the Shawnees nation, they had been abſent about two years and a half. Griffith ſuppoſed that when they travelled they went at the rate of about fifteen miles per day.

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He ſtaid but a few months with the Indians after his return, as a favourable opportunity offered itſelf to him to reach his friends in Virginia. He came with a hunting party of Indians to the head waters of Coal-river, which runs into New-river not far above the falls. There he left the Shawnees and eaſily reached the ſettlements on Roanoke.

Mr. Childs knew him before he was taken priſoner, and ſaw him a few days after his return, when he narrated to him the preceding circumſtances. Griffith was univerſally regarded as a ſteady honeſt man, and a man of ſtrict veracity. Mr. Childs has always placed the utmoſt confidence in his account of himſelf and his travels, and has no more doubt of the truth of his relation, than if he had ſeen the whole himſelf. Whether Griffith be ſtill alive or not he does not know.

Whether his ideas be correct or not, we ſhall probably have a better opportunity of judging on the return of Captains Lewis and Clark; who, though they may not penetrate as far as Griffith alledged that he had done, will probably learn enough of the country to enable us to determine whether the account given by Griffith be fiction or truth.

I am, Sir,

Your humble ſervant,

HARRY TOULMIN.

Frankford, Dec. 12, 1804.

Additional Obſervations and Conjectures by the Editor.

THE ſtory of a Welch colonization of America has excited much curioſity, both in Europe and the United States: By many it is believed, while by others it is thought unworthy of any attention. By reaſon of the preſent rapid progreſs of ſettlement in America, the time cannot be remote when the truth or falſity of this ſtory will be completely eſtabliſhed. In the meanwhile I do not heſitate to conjecture, *that no traces of the deſcendants of the Welch prince will ever be diſcovered in the weſtern parts of North America.*

It

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It may not be improper to notice the tale upon which so many persons in Europe at least rest their hopes of proving, in the most satisfactory manner, that the Welch have contributed to the peopling of America.

David Powel, a Welch historian, informs us, that on the decease of Owen Guyneth, king of North Wales, a dispute arose among his sons concerning the succession to the crown; and that Madoc or Madog, one of the sons, "weary of this contention, betook himself to sea, in quest of a more quiet settlement *." We are informed, that "he steered due west, leaving Ireland to the north, and arrived in an unknown country, which appeared to him so desirable, that he returned to Wales, and carried hither several of his adherents and companions. After this neither Madog nor his companions were ever heard of more. The voyage of Madog is said to have been performed about the year 1170.

I have not seen Powel's work, but I learn that this historian, who lived in the reign of Queen Elizabeth, and consequently at a great distance of time from the event which he records, adduces no better authority in support of the voyage than a quotation from a Welch poet, "which proves no more than that he (Madog) had distinguished himself by sea and land †." Some few Welch words, such as *gurando*, to hearken or listen, &c. are very feebly or unfortunately adduced by Powel, as circumstances favourable to the truth of the Welch emigration.

When we consider "that the Welch were never a naval people; that the age in which Madog lived was peculiarly ignorant in navigation;" that the compass was then unknown; the story of the voyages of the Welch prince must I think be considered as extremely improbable. I am of opinion with Mr. Pennant, that "the most which they could have attempted must have been a mere coasting voyage."

But it may be said, we must appeal to facts; and that independently of the verses of the Welch poet, and the arguments of the Welch historian, it seems highly probable that a colony of white people who speak the Welch language, does actually exist in the western parts of North America.

* Dr. Robertson.

† Pennant's Artic Zoology, Introduction, p. cclxiii. &c.

I cannot,

I cannot, I must confess, adopt this opinion. I readily allow, that the relations published by Mr. Toulmin and many other persons, both in Europe and in America, are extremely curious. But these relations are very inconsistent with one another, particularly in what relates to the actual state of improvement of the supposed Welshmen. By some we are told they are very far advanced in improvement; by others that their improvement is not at all greater than that of the Red-men or Indians of America. At one time, they are said to be in possession of manuscripts (and even printed books) at another time nothing of this kind is found among them. It must be confessed that Maurice Griffith's relation is, in several respects, more plausible than that of any preceding traveller; but it is not unincumbered with inconsistencies, which I do not deem it necessary to notice in this place. His assertion "that the white men of the Missouri speak pure Welch," even though this assertion be qualified by the observation that "they occasionally make use of a few words with which he was not acquainted," is to me one of the most improbable things that have ever been related of these people. His silence about their religion is altogether inexcusable. One would suppose that a person of Griffith's inquisitive turn of mind, would hardly have omitted to make some inquiries respecting the religious institutions of a people, whom he considered as his countrymen. If these people be the descendants of Madog, *some* traces of the Christian religion may be expected to be discerned among them; for I think it requires many centuries to entirely efface from the memory of a people all vestiges of their religion, especially from a people so tenacious of their language, and so little disposed to intermix with their neighbours, as the Welch Indians are represented to be.

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But Griffith's relation is, I think, worthy of some attention. I even think it not altogether improbable that future researches will establish the fact, that there does exist in the western parts of North America a race or nation of men, whose complexion is much fairer than that of the surrounding tribes of Indians, and who speak a language abounding in Welch or Celtic words. But the *complete* establishment of these two points would not prove the establishment of the truth of the assertion, that Prince Madog had ever made a voyage to America, or that

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that a colony of Celts had at any period prior to the *discovery of America by Columbus*, passed into this hemisphere *from Britain*.

It may be thought, from the statement published by Dr. Williams and some other writers on the subject, that the belief of the existence of a race of Welch Indians in America is generally admitted by the Welch Indians and others. But this is far from being the case. The late Mr. M'Gilivray, a man of no inconsiderable powers of mind, and whose curiosity was by no means confined to his own relatives, the Muscohge, or Creek Indians, informed me, in the year 1790, that he knew nothing of the existence of any white people in the tract of country beyond the Mississippi.

The following is an extract of a letter (dated *Downing, June* 14, 1792) from my learned and excellent friend the late Mr. Thomas Pennant of Wales.

"My countrymen are wild among the Padoucas, or Welch Indians, descendants of Madog, now seated about the upper parts of the Missouri. I am rather in disgrace, not having the warmest hopes of their discovery. Pray what is your opinion and that of your philosophers?"

In answer of the above I wrote a letter, of which the following is a part:

"I have heard a great deal about the Welch Indians. I very early imbibed your opinion, as delivered in your Arctic Zoology *, and mentioned you on the subject in a little work † which I published in England at the age of * * * *. I do not know whether you have seen that work. I do *not* mean to hint that it is worthy of your attention. I certainly think there is some foundation for the story; but I have no doubt but the whole affair will turn out very different from a discovery of Madog's descendants in America.

"I have said, that I think there is some ground for the story. I shall explain myself. You know that many of the first visitors of the new world were struck with the resemblance which

* See the introduction to the work, pages 263, 264.

† Observations on some parts of natural history; to which is prefixed an account of several remarkable vestiges, of an ancient date, which have been discovered in different parts of North America. Part I. London, 1797.

subsists

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subsists between some of the American nations and the Jews. Some Hebrew words were found in this continent, as they have been every where else. The Americans were now said to be the descendants of the Jews, and Adair laboured very hard to prove the matter in a ponderous quarto which few people read, because it is big with system and extravagance, though, indeed, it contains some curious and accurate matter. In like manner, in the languages of some of the American tribes there are found some words which are a good deal analogous to words in the languages of the ancient Celts. Wafer, who was a very respectable observer, if we consider his occupation in life, mentions the coincidence he found between the language of the Indians of Darien and that of the Highland Scots; and I could produce instances of their coincidence. Some Greek words are also found in certain of the American languages. I would not strain a point so much as some writers have, who mention the coincidence which subsists between the Greek *Theos* and the Mexican *Teotl.* The word *Potowmack,* which is the name of one of our great rivers, is a good deal like the Greek *Potomos* ‡. These words (perhaps they are accidental resemblances) have given rise to some of the numerous theories which we have had concerning the peopling of this great continent: and I doubt not that some * * * * or person who understood the Welch language, finding Celtic words (a language spoken by the Welch) among the Americans, in the fulness of his zeal would bring his countrymen among the Padoucas, Apaches, &c.

"Such, I believe, has been the origin of this wonderful story. I presume, that, were an ignorant Highlander to visit the Darien Indians, or some other American tribes, he would fancy himself among his countrymen, whom painting, exposure to the sun, &c. he might suppose had exalted or degraded to their present tinge. I lately conversed with an old Highlander, who said, that the Indians speak the Highland language. Some Highland words were mentioned by him;

‡ The Abbé Molina (in his Compendio de la Historia Civil del Reyno de Chile, &c. Parte Segunda, p. 334, 335.) has pointed out some very striking instances of resemblance between the Greek and Chilese languages. He has also pointed out some resemblance between the Latin and the Chilese.—February 19, 1805.

* * * * one

**** one word **** I recollect, the word *teine*, which in the Highland language, he said, signifies fire: now our Delaware Indians call fire *teriday*; the resemblance is found is certainly not small. The Celts have, undoubtedly, been very widely spread over the globe: I believe they existed in this country, and that their descendants are some of the present tribes *. That Celtic words should be found among the Americans, when Celtic words are to be found almost every where else, is not I think to be wondered at."

IX.

Account of an improved Sheep-Fold, contrived and constructed by THOMAS PLOWMAN, *Esq. of Broom in Norfolk, and communicated by him to the Society for the Encouragement of Arts* †.

Advantages of the new sheep-fold.

THE model of Mr. Plowman's Sheepfold was forwarded to the Secretary of the Society of Arts last year with a letter describing its properties and construction. It is made on an improved and very simple principle, combining many advantages over the old and expensive method of folding by hurdles; and as the whole fold can be removed with ease at all times, it is found peculiarly useful in feeding off turnips on the land in frosty weather, when hurdles cannot be used; and, as the saving of labour in agriculture is a leading object, he has no doubt of seeing it, in a very few years, generally adopted.

Durability.

The expence, in the first instance, will exceed that of hurdles, for the same given quantity of sheep; but having had one in use nearly three years, he is satisfied the saving will be very considerable: for, before he adopted this method of folding, he lost from thirty to forty nights folding in the year, owing to the land being hard in dry seasons, such as the

* Very considerable fragments of the Celtic dialects are still preserved in America; particularly, if I do not mistake, among the Ranticokes and the Katalba or Katawbas. *February* 19, 1805.

† The Society awarded the gold medal for this useful improvement, and inserted his account.

two

two last; which renders folding almost impracticable, as they never can be set without great labour and destruction of hurdles. He is also clearly of opinion, that the stock of sheep will be greatly increased when this method of folding becomes more known; and that it will enable many small farmers to keep from 50 to 100 sheep, who now are deterred from it, on account of the small quantity of feed they have, not answering to keep a man for that purpose only; but by this plan, they may keep a boy at 3s. or 3s. 6d. per week, who can attend on 100 or 200 sheep, and move the fold himself without any assistance. In heavy gales of wind it frequently happens that hurdles are blown down, and the sheep, of course, being at liberty to range over the crops, do incalculable mischief; which cannot happen with this fold.

Saving of hurdles; and greater profit in feeding and keeping sheep.

It is easily moved, and not liable to be blown down;

In some counties in England, where hogs are folded, great difficulties are experienced for want of stowage, for them to feed off winter tares, &c. &c. as they root up every stake or hurdle; but from having tried the experiment, the inventor is certain his fold will keep them in, and defies their attempts to displace it.

resists hogs.

From this drawing, which corresponds with the model, and from the description, it is seen that an astonishing quantity of time is saved; for one man can remove a fold to contain 300 sheep with ease in five minutes, which, by the old method, frequently takes some hours to accomplish.

Certificates of gentlemen, who use these new folds, were sent to the society, among whom is that of his Grace the Duke of Bedford.

When the fold is wanted to be used on very hilly ground, it is best to begin at the top, and work it down to the bottom, for the ease of removing it, and then draw it up again with a horse. This, however, the inventor has never had occasion to do; for the land in his county is ploughed in a contrary direction, and the fold is worked in the same course as the ridges. By this mean, the inconvenience is avoided of crossing the furrows, and they are also a guide to keep the fold in a straight direction.

Method of placing it on hilly grounds.

With respect to the sheep getting under, he does not recollect that circumstance to have ever happened, nor does he conceive that any land, which is cultivated can be so uneven as to admit of it.

Description with reference to the drawing.

Description of the Sheepfold.

Plate XI. Fig. 1. Shows one division or part of this fence twenty-one feet long, and three feet eleven inches high, composed of the following parts:

A. A top rail three inches deep and two inches thick. B. The upper bar, three inches deep, and three-quarters inch thick. CC, The two lower bars, four inches by three-quarters of an inch, which, with the upper bar, are morticed through the uprights. DDDD, Which uprights are oak, three inches by two inches. E, The lower bar, three inches by three. F. An upright bar, with the horizontal bars halved into it. GG, Two oak uprights, three by two inches.

Fig. 2. Shows the oak uprights GG. H, The axletree, three inches by three, and three feet between the wheels. I, An oak knee, which connects the uprights GG with the axletree, by means of two screws and nuts.

Fig. 3. A plan, in which the axle H is shown with two arms KK at right angles to H, which are made to act as pivots to the wheels, when intended to be moved in a direction at right angles to the bars.

Fig. 4. Is a view of the same parts described in fig. 3. The wheels marked W, in all the figures, are of cast iron, and cost 3s. 6d. each.

X.

Anecdotes of an American Crow. By WILLIAM BARTRAM*.

Anecdotes of a crow.

IT is a difficult task to give a history of our crow. And I hesitate not to aver, that it would require the pen of a very able biographer to do justice to his talents.

Before I enter on this subject minutely, it may be necessary to remark, that we do not here speak of the crow collectively, as giving an account of the whole race, since I am convinced that these birds differ as widely as men do from each other in point of talents and acquirements, but of a particular kind of that species, which I reared from the nest.

* From the Philadelphia Medical Journal, Vol. I. part I.

He

He was, for a long time, comparatively, a helpless dependant creature, having a very small degree of activity or vivacity, every sense seeming to be asleep, or in embryo, until he had nearly attained his finished dimensions and figure, and the use of all his members. Then we were surprised and daily amused with the progressive developement of his senses, expanding and maturating as the wings of the youthful phalaena, when disengaged from its nympha shell. Anecdotes of a crow.

These senses however, seemed, as in man, to be only the organs or instruments of his intellectual powers, and of their effects, as directed towards the accomplishment of various designs and the gratification of the passions.

This was a bird of a happy temper and good disposition. He was tractable and benevolent, docile and humble, whilst his genius demonstrated extraordinary acuteness and lively sensations. All these good qualities were greatly in his favour, for they procured him friends and patrons, even among men whose society and regard contributed to illustrate the powers of his understanding. But what appeared most extraordinary, he seemed to have the wit to select and treasure up in his mind, and the sagacity to practice, that kind of knowledge which procured him the most advantage and profit.

He had great talents, and a strong propensity to imitation. When I was engaged in weeding in the garden, he would often fly to me, and after very attentively observing me in pulling up the small weeds and grass, he would fall to work, and with his strong beak pluck up the grass; and the more so, when I complimented him with encouraging expressions. He enjoyed great pleasure and amusement in seeing me write, and would attempt to take the pen out of my hand, and my spectacles from my nose. The latter article he was so pleased with, that I found it necessary to put them out of his reach when I had done using them. But one time, in particular, having left them a moment, the crow being then out of my sight, recollecting the bird's mischievous tricks, I returned quickly and found him upon the table, rifling my inkstand, books, and paper. When he saw me coming, he took up my spectacles and flew off with them. I found it vain to pretend to overtake him; but standing to observe his operations with my spectacles, I saw him settle down at the root of an apple-tree, where, after amusing himself for awhile, I observed

Anecdotes of a crow.

that he was hiding them in the grafs, and covering them with fticks and chips, often looking round about to fee whether I was watching him. When he thought he had fufficiently fecreted them, he turned about, advancing towards me at my call. When he had come near me, I ran towards the tree to regain my property. But he judging of my intentions by my actions, flew, and arriving there before me, picked them up again, and flew off with them into another apple tree. I now almoft defpaired of ever getting them again. However I returned back to a houfe a little diftance off, and there fecreting myfelf, I had a full view of him, and waited to fee the event. After fome time had elapfed, during which I heard a great noife and talk from him, of which I underftood not a word, he left the tree with my fpectacles dangling in his mouth, and alighted with them on the ground. After fome time, and a great deal of caution and contrivance in choofing and rejecting different places, he hid them again, as he thought, very effectually in the grafs, carrying and placing over them chips, dry leaves, &c. and often pufhing them down with his bill. After he had finifhed this work, he flew up into a tree hard by, and there continued a long time talking to himfelf and making much noife; bragging, as I fuppofe, of his achievements. At laft he returned to the houfe, where not finding me, he betook himfelf to other amufements. Having noted the place where he had hid my fpectacles, I haftened thither, and after fome time recovered them.

This bird had an excellent memory. He foon learned the name which we had given him, which was Tom; and would commonly come when he was called, unlefs engaged in fome favourite amufement, or foon after correction; for when he had run to great lengths in mifchief, I was under the neceffity of whipping him, which I did with a little fwitch. He would in general bear correction with wonderful patience and humility, fupplicating with piteous and penitent cries and actions. But fometimes when chaftifement became intolerable, he would fuddenly ftart off, and take refuge in the next tree. Here he would confole himfelf with chattering and adjufting his feathers, if he was not lucky enough to carry off with him fome of my property, fuch as a pen knife, or a piece of of paper; in this cafe he would boaft and brag very loudly. At other times he would foon return, and with every token of penitence

penitence and submission approach me for forgiveness and reconciliation. On these occasions he would sometimes return and settle on the ground near my feet, and diffidently advance with soft soothing expressions, and a sort of circumlocution, and sit silently by me for a considerable time. At other times he would confidently come and settle upon my shoulder, and there solicit my favour and pardon with soothing expressions and caressing gesticulations; not omitting to tickle me about the neck, ears, &c. Anecdotes of a crow.

Tom appeared to be influenced by a lively sense of domination (an attribute prevalent in the animal creation) but nevertheless his ambition, in this respect, seemed to be moderated by a degree of reason or reflection. He was certainly by no means tyrannical or cruel. It must be confessed, however, that he aimed to be master of every animal around him, in order to secure his independence and his self preservation, and for the acquisition and defence of his natural rights. Yet in general he was peaceable and social with all the animals about him.

He was the most troublesome and teazing to a large dog whom he could never conquer. This old dog from natural fidelity and a particular attachment commonly lay down near me when I was at rest, reading or writing under the shade of a pear-tree in the garden near the house. Tom (I believe from a passion of jealousy) would approach me with his usual caresses and flattery, and after securing my notice and regard, he would address the dog in some degree of complaisance, and by words and actions; and if he could obtain access to him, would tickle him with his bill, jump upon him, and compose himself for a little while. It was evident, however, that this seeming sociability was mere artifice to gain an opportunity to practice some mischievous trick, for no sooner did he perceive the old dog to be dozing, than he would be sure to pinch his lips, and pluck his beard. At length, however, these bold and hazardous achievments had nearly cost him his life, for one time the dog being highly provoked, he made so sudden and fierce a snap, that the crow narrowly escaped with his head. After this Tom was wary, and used every caution and deliberation in his approaches, examining the dog's eyes and movements, to be sure that he was really asleep, and at last would not venture nearer than his tail, and then by slow,

silent, and wary steps, in a sideways or oblique manner, spreading his legs and reaching forward. In this position he would pluck the long hairs of the dog's tail. But he would always take care to place his feet in such a manner to be ready to start off when the dog was roused and snapped at him.

It would be needless (observes my ingenious friend in the conclusion of this entertaining account of the crow) to recount instances of this bird's understanding, cunning, and operations, which certainly exhibit incontestible demonstrations of a regular combination of ideas, premeditation, reflection, and contrivance, which influenced his operations.

XI.

An Account of the Seiches of the Lake of Geneva. By M. VAUCHER *.

Sudden and irregular rise and fall of the lake of Geneva called seiches.

THE inhabitants of the banks of the lake of Geneva, designate by the name of seiches certain sudden and irregular changes which take place in the level of the waters of the lake, and have no relation with the regular and annual increase produced by the melting of the snows. This phenomenon was described at the beginning of the last century. Fatio de Duilers in the 2nd vol. of Spon's History of Geneva; and afterwards by Jalabert in the Academy of Sciences; Serre in the Journal de Savans, Bertrand, and by De Saussure in the 1st vol. of his Travels in the Alps. But though several of these philosophers have attempted to explain the fact, as we shall hereafter remark, yet no one has considered it with precision, and as a general phenomenon. The editors of the Bulletin des Sciences, from whose excellent sheet I take the present account, have followed Mr. Vaucher, and afterwards present the different explanations. The numerous observations of that philosopher have led him to the following general results.

Particular detail of the facts; they are observed in other lakes.

1. The seiches are not peculiar to the lake of Geneva, they are also observed in the Lakes of Constance, Zurich, Annecy, Neuf-chatel, and in the lake Major, and there are strong reasons to think that they exist in most lakes, though they may not have been sufficiently observed.

* From the Bulletin des Science, No. 96.

2. It

2. It appears, however, to be true, that the phenomenon is more remarkable in the lake of Geneva than any where else that it has been observed. In fact, the level of the waters of Leman lake have been several times observed to rise at a given place in the course of 15 or 20 minutes, three, four, and even five feet, and to subside some time afterwards, whereas the strongest seiches observed in other lakes, have been four or five inches in the lake of Constance, eighteen lines in that of Zurich, four or five lines in that of Annecy, and only a few lines in the lake of Neuf-Chatel and lake Major. But most strikingly in the lake Leman.

3. In all these lakes, particularly in that of Geneva, the seiches are most sensible in that part of the lake which is nearest the outlet of its waters. Accordingly they are no more than one or two inches, at the distance of two leagues from Geneva, and at the extremity near where the lake receives its waters the seiches of the lake of Geneva are not stronger than those of the other lakes here mentioned. More considerable near the place of efflux,

4. In these different lakes they are most sensible in places where the lake is remarkably narrow. and where the shores are not far asunder;

5. The seiches may take place indifferently at all seasons of the year, and at any hour of the day; but in all the lakes it has been observed, that they are more frequent in the day than in the night, and in the spring and autumn, than in the winter or summer. they happen at all times and seasons;

6. It has been observed in particular in the neighbourhood of Geneva, that the strongest seiches take place at the end of the summer, that is to say, at the time of the greatest elevation of its waters. but most strikingly when the waters are highest;

7. The seiches are extremely frequent, but they are usually a few lines, or at most only a few inches, in which cases they cannot be perceived without exact apparatus to observe the level of the lake. It is from a want of this observation that they have been supposed to be very rare, as those seiches only could be observed without apparatus which varied several feet.

8. The seiches take place without any agitation or motion of undulation or current in the surface of the fluid. attended with no agitation,

9. Their duration is very variable, seldom exceeding twenty or twenty-five minutes, and often much less. and do not last long;

10. This

they seem to be dependent on the weight of the atmosphere, and are thought to foretel rain.

10. This phenomenon takes place in all temperatures, but in general it results from very extensive tables, that the seiches are more frequent, and more extreme, the more variable the state of the atmosphere. Remarkable variations of the barometer have been observed to correspond with considerable seiches, and it is an opinion generally received among the fishermen, that the seiches are a sign of change of weather. In particular, they have been observed to be very strong when the sun comes to shine very strongly on a spot, a short time before obscured by a thick cloud.

Explanations by various authors.

After this exposition of the phenomenon, some notion may be formed respecting the value of the different explanations. M. Fatio attributes the seiches to violent gusts of wind which drive the waters into the narrowest part of the lake. Mr. Jalabert attributes them to some sudden encrease of the Arve, which falling into the Rhone at a short distance from the lake, and entering the river at a considerable angle, may in fact, sometimes stop its course for a short period, and in that manner raise the waters of the part of the lake nearest Geneva; lastly, Mr. Bertrand thinks this phenomenon to be occasioned by electrical clouds which attract the waters of the lake, and produce oscillations more sensible, the nearer its opposite banks may be to each other. Without dwelling on the insufficiency of these three hypothesis to account for all the different facts before mentioned; Mr. Vaucher observes, that the true explanation ought to be two-fold; namely, general in order to shew the cause of those less considerable seiches which are observed in all the lakes, and over the whole of their surface; the other must be local, and explain why this phenomenon is much more sensible at the western extremity of the lake of Geneva, than in any other known place.

Mr. Vaucher ascribes them to atmospheric pressure acting more strongly on one part of the lake than at the place of rise.

With respect to the first, Mr. Vaucher ascribes it to the frequent variations which are sensible in the weight of different columns of the atmosphere, and consequently in the pressure it exerts on different points of the surface of lakes *. We may easily conceive, that if the weight of the atmospheric column be speedily diminished in a given part of a lake, with-

* This cause was before indicated concisely by De Saussure, in his first vol. of Travels in the Alps.

out

not the same thing happening over the rest of the surface; or still more if the weight should be augmented upon that remaining surface, the water will be forced to rise in that last place, and will again descend when the atmosphere shall have resumed its equilibrium. It is known, in fact, that these variations of the barometer are so frequent, that it can never be said to be exactly stationary; it is known, that they can be produced by changes of temperature, and De Saussure has calculated that a diminution of three degrees in the column of air will account for a variation of 0,85 of a line in the barometer. It is known, that these variations are most frequent in mountainous countries in autumn and in spring, and previous to storms, circumstances which coincide with the greater frequency of seiches at those times. This general cause tends to explain the slight variations of level which are common to all the lakes; it is even of such a nature as to be applicable to all extended surfaces, and it is therefore probable, that these variations of level likewise take place in the sea, independant of the flux and reflux, which may have hitherto prevented their being observed. The variations in the weight of the atmosphere may perhaps contribute to those sudden and local elevations of the waters of the sea, which have all been indistinctly considered as of the nature of water-spouts. The same cause ought likewise to act on rivers, but instead of raising or diminishing their level, it ought, according to Mr. Vaucher, to produce a momentary acceleration or retardation of their course; an observation difficult to be made, and not hitherto attempted.

—and he supposes the greater rise in the lake of Geneva to be caused by its peculiar figure.

As to the second part of the explanation, namely, that which should account for the great intensity of the phenomenon at the extremity of the Leman lake, near Geneva, Mr. Vaucher recurs to two circumstances peculiar to that lake, and which are found in a less degree in those of Zurich and Constance, where the seiches are most remarkable after those of the lake of Geneva; namely, the contraction of a lake in a given place, and the descent of its waters towards the place of their discharge. With regard to the first of these circumstances, it will be sufficient, if attention be paid to a chart of the Leman lake, to shew that it is very remarkably contracted at its western extremity, so that at half a league distance from Geneva, it has not one third of the breadth of that before Thonon. Now we

we may compare a lake of this form to a syphon full of water, of which the branches should very much differ in diameter; and it will be evident that if, for example, their inequality being as fourteen to one, the smallest branch should suddenly receive by the augmentation of the atmosphere a surcharge equal to that which depresses the barometer one line, it would fall 14 lines, and the water which would be driven into the great branch would raise its surface only one line; whereas, on the contrary, a surcharge which should depress the level of the great branch only one line, would raise it for a moment fourteen in the smaller. The effect would be double if at the same time the weight of the atmosphere should diminish on one of the branches, and encrease on one of the other. We may therefore admit that in lakes, the breadth of which is remarkably contracted in some part, the influence of the variations of the atmosphere to produce seiches will be greater in the narrow than in the wide part.

And also by circumstances attending the flowing off of the waters.

A like effect will take place according to Mr. Vaucher, by reason of the inclination observable in that part of the surface of the lake near the place where it discharges its water. He remarks that every particle of a liquid on a slope may be considered as solicited by two forces; one which tends to raise it to the level of the superior part of the slope or the reservoir, and the other which urges it in the direction of the current. If by the sudden depression of the superior fluid the current be for a moment suppressed, the particle will no longer find itself urged but by the first of these forces, and will rise towards its ancient level, and soon afterwards descend. Now, as we have before seen, all the parts of lakes which have very perceptible seiches have a remarkable slope; this slope is naturally more considerable at those times of the year when the waters are highest, and these are the periods when the seiches are most striking in the neighbourhood of Geneva.

Singular appearance which sometimes occurs that the surface of the lake is partly smooth and partly agitated.

Independent of the phenomenon of the seiches, the lake of Geneva and most other lakes afford two other singular phenomena; the one is known by the fishermen of the Leman lake by the name of fontaines. This takes place when the surface of the lake, instead of being uniformly calm or uniformly agitated, is seen to have certain parts calm and certain parts agitated, which are often mixed among each other in a thousand manners, and always very distinct. This fact seems to indicate

cate the different atmospheric columns, though very near each other, may some of them be agitated and others calm. This appearance of the surface of the lake is considered by the fishermen as a sign of rain.

The second phenomenon of which Mr. Vaucher speaks, consists in certain sonorous distant explosions or noises which resemble those of the discharge of artillery, and are sometimes heard in the fine summer evenings. This phenomenon is rare, but is nevertheless affirmed by several inhabitants near the lake of Geneva. It also takes place in the lake of Zurich according to Mr. Escher, and in that of Baikal according to that of Mr. Patrin. Mr. Escher asserts that half or three quarters of a minute after having heard one of these noises he saw a bubble of air about a foot in diameter rise out of the lake of Zurich.

Another phenomenon resembling the distant noise of artillery.

Annotations.—W. N.

It does not seem to me that any of the causes yet pointed out are sufficient to account for the effect of the seiches. Sudden or strong blasts of wind could scarcely operate in this way so partially as that the existence of such squalls should not at the same time have fixed the attention of the common people as well as of the more accurate observers who have noticed these changes. It is perhaps equally difficult to suppose such unheeded variations to take place in the Arve sufficient to account for these very remarkable changes in the lake. Mr. Bertrand's electrical hypothesis refers us to a class of appearances too little understood to be admitted, otherwise than in the way of loose conjecture; besides which, it must be remarked that the agency of electrical clouds is much more generally directed to mountains than to the valleys in which lakes must necessarily have their situation. Much ingenuity is lastly shewn by Mr. Vaucher in his explanation, which nevertheless requires us to admit of atmospheric columns considerably differing in weight and occupying very small extent of surface. If this be even admitted as possible, yet strong doubts may surely be entertained as to its probability. It appears to me that the object in question admits of an easy solution upon other principles, and also that his explanation is grounded on positions not consistent with the known laws of statics.

Objections to the theories which have been offered respecting the seiches,

—particularly that of Mr. Vaucher's of atmospheric pressure.

This ingenious author assumes as the conditions of his general theory that the lake should consist of two portions of water,

Recapitulation of his facts and deductions.

water, one much more extenſive than the other, and connected by a narrower portion or gut. He then ſtates that if the atmoſpheric preſſure be greater upon the larger ſurface than on the ſmaller, the firſt will be depreſſed and the latter will riſe, and that the difference of elevation in each ſurface occaſioned by the paſſing of any given quantity of water will be greater the ſmaller the ſurface.

No atmoſpheric change can make a greater alteration in the lake than the correſpondent riſe and fall of a water barometer, which is much leſs than really takes place.

This is very true; but it can in no caſe happen, that the difference between the level of one water and the other can amount to a greater quantity than that of a water barometer, by a like change, namely, about fourteen lines for every line of variation in the common barometer. That is to ſay, if the barometer were to riſe and fall again through half an inch, in the ſhort time of a ſeiche, which I believe ſcarcely if ever happens, the ſeiche itſelf could not riſe above ſeven inches. The whole range of variation in the barometer could only cauſe a riſe of three feet and a half inſtead of five which ſometimes happens.

Another theory offered; that the ſeiches depend altogether on the rapidity of ſupply and facility of diſcharge.

I would venture to conjecture that this phenomenon is one among the numerous oſcillatory proceſſes which take place when two variable natural powers are oppoſed to each other in the production or modification of any event. Moſt ſmall lakes are formed by the enlargement of a river, by which the lake is ſupplied at one end and evacuated at the other. The quantity of water in the lake itſelf will, in theſe circumſtances, be always more than would be ſufficient to fill its capacity, taken from the level of the loweſt point of diſcharge. How much more it may be than this quantity will depend upon the ſtreams which enter and paſs out. An increaſe in the quantity of ſupply will keep the level higher, and ſo likewiſe will any increaſe in the obſtacles to its flowing off; and on the contrary, if the ſupply be diminiſhed, or if the facility of diſemboging be encreaſed, the level will be depreſſed. Theſe effects will take place moſt ſtrikingly at firſt at that end of the lake where the efficient cauſe operates. When any change has once taken place, ſuch as that of the depreſſion, it will continue for a ſhort time after the cauſe has ceaſed to act; ſo that the depreſſion would itſelf be followed by a riſe, even if the circumſtances which cauſed it were not alſo ſubject to a like variation.

This effect is ſeen in brooks and mill-ponds.

Changes of this kind, on a ſmall ſcale, are obſervable in mill-dams, and even in the ſmooth places in brooks or rivulets, as may

may be observed by taking notice of some part of the bank where a gently rising sand may render the changes of level more conspicuous. The variableness of the weather at the spring and autumn, by occasioning more frequent changes in the quantities of water, and consequently in the state of the rivers above and below the lakes in question must render the seiches more frequent and extreme at these times. They will also be most evident at the ends of a long lake; and the other circumstances will be modified by events that for the most part would require to be ascertained by observations of the local circumstances and events on the spot.

It must be more considerable in spring and autumn.

The distinct portions of rough and smooth surface called *fontaines*, which are observed on the lakes, are very strikingly seen at sea whenever a breeze springs up after a dead calm. This effect is very remarkable, and may perhaps be accounted for on the supposition that the incipient motions of the air may be attended with eddies that may act more strongly on one part of the surface than another. This however does not seem reconcileable with a certain steadiness of appearance with which the smooth and rough surfaces continue distinct from each other for certain lengths of time. I am not much satisfied with the conjecture which offered itself to me, or which may have been mentioned by some other person when I was at sea many years ago; but it at least deserves to be noticed here. It is well known that the wind scarcely takes hold of water which is covered with any oily film, and from the experiments of Franklin and others we have learned that a single drop of oil will rapidly spread over a large surface of water, and cause all the small primary waves to subside, rendering the surface extremely smooth. It seemed to me not unlikely that oily matter from animal remains might rise to the surface of the sea during a calm and spread itself irregularly over certain parts, which would continue smooth for a considerable time after the light commencing breeze had ruffled the other parts. I think from recollection that this appearance could not have lasted more than a quarter of an hour; but it is very common, and I often saw it. May not a similar cause produce the appearance in the lake of Geneva.

The distinct patches of smooth and rough surface on the lake are very usual at sea after a calm.

Supposed to arise from oily matter on the surface of the water,

—of the lake also.

The sonorous reports resembling discharges of artillery seem very likely to arise from the extrication of gas at the bottom of the water, which rises and breaks at the surface. I have no

The sonorous reports resembling artillery supposed to be made by gases.

no remark to make on this subject, but advert to it principally with a view to mention an effect not generally known, but calculated to shew the agitation which a small quantity of ascending air can produce in water. If a swimmer fill his lungs with air by inhaling as much as possible, and then dive down or descend into the water to the depth of fourteen, twenty or more feet, and when at that depth slowly blow the air out of his mouth, he will himself hear a roaring noise, and the spectators will see with surprise the surface of the water raised into a round or conical mass about a yard in height, with the water flowing round on all sides over a surface of seven or eight square feet. I have little doubt but that the noise of this rising column of water with the breaking of the bubbles of air would be very remarkable in one of the still evenings or nights of summer, when the effect of noises is remarkably more impressive than when the louder sounds of the day render them less observable, and in many instances altogether inaudible.

Remarkable effect of agitation in water, produced by air blown slowly through the lungs at the depth of several feet beneath the surface.

XII.

Experiments to ascertain the best Colour for marking the Heads of Pieces of Cotton or Linen in the rough, which shall be capable of resisting the Operations of Bleaching, as well as the most complicated Processes of Calico Printing, without spreading beyond the Limits of the Impression. By Mr. HAUFFMANN.*

IN order that a colour may be proper to mark piece goods of every kind it is requisite that it should contain no substance or drug capable of solution in alkalies; it is equally necessary that its component parts should not become white by oxygenation, and that they should remain insoluble in acids sufficiently strong for the bleaching processes, as well as for the operations antecedent to the calico printing.

Properties required in a good marking colour or ink.

Colours composed of drying oil cannot therefore, as I have found, be useful in these kind of marks, because they are not only attacked by alkaline and soapy liquids, but likewise because they dry slowly, and by spreading beyond the limits of impression, very often occasion spots.

Oil colours are bad because they yield to alkalies, &c.

* Annales de Chimie, LIII. 208.

If the colours of spirituous varnishes were not subject to the inconvenience of too speedy evaporation and drying they would be inadmissible on another account, namely, that the turpentine and resins are easily converted into soap. Gum copal is equally unfit for marking colours, because it quits the piece by simple ebullition in water. But as the varnish which I have made defends vessels of copper or any other metal from the action of acids of a certain strength as well as from that of the atmosphere, I have thought it might not be unacceptable to describe its composition in this place. To obtain this varnish from copal as pale and as clear as water, this gum must be reduced to very fine powder and exposed with twelve parts of the finest oil of turpentine for several days, or until it shall be completely dissolved at a moderate heat on a sand bath in a capsule of brass, stone ware, or porcelain, taking care to stir it as often as possible with a rod of glass. At the moment when the consistence of syrup begins to take place, the entire solution of the copal is effected by agitation, particularly if a small quantity of oil of turpentine be added from time to time to supply the loss by evaporation. Three fourths of the oil of turpentine which is lost by evaporation when open vessels are used, may be saved by performing the process in a long necked mattrass, which is to be exposed on a sand bath a sufficient time to complete the solution of the copal, and shaking it very often. The varnish obtained by either of these methods becomes yellowish if the heat be urged too strongly; and as by its too glutinous consistence it would be difficult in its application, it is convenient, instead of diluting it with oil of turpentine, to mix it with one fourth or one fifth part of its weight of alcohol, taking care not to use too much, because an excess would render it of a milky white by the precipitation of part of the copal, which cannot admit in its solution more than a certain quantity of alcohol without precipitating. Vessels of brass or of any other metal may receive one, two, or three coatings of this varnish, and must be each time well dried in the oven. After this treatment they may be washed with boiling water without injury, and may even be exposed to a still greater heat without the varnish coming off; but these vessels must not be rubbed with sand or other hard bodies.

Varnish colours are equally faulty in this respect.

Copal yields to boiling water.

Process for making a good varnish. Copal in powder is dissolved by heat and agitation in oil of turpentine.

The copal varnish is to be diluted with alcohol.

Metallic vessels having this varnish baked upon them may be exposed to boiling water without injury.

By means of oil of turpentine, which evaporates and dries less speedily than alcohol, I succeeded in making a black composition

An oily compound for marking linen and other goods.

position, which I expected might be used with advantage in marking piece goods. For this purpose nothing more is needful than to dissolve slowly on the sand bath, and with constant agitation. One fourth of its weight of asphaltum or bitumen, judaicum well pounded, and afterwards to mix as much lamp black, or any other dark coloured mineral in fine powder, such as black lead, galena, or the like. This colour may be had more or less thick, by due proportions of the oil of turpentine and bitumen; it prints very well without running, if the proper proportions be attended to, and a little oil of turpentine be added when it becomes too thick. This bituminous colour supports the action of alkalies and of oxigen very well, and resists all acids of moderate strength.

As I thought it unnecessary to continue my experiments on oil colours, I made my experiments on watery compounds in the following order.

Section I.

First marking process. An impression is made of a solution of sulphate of manganese thickened with gum, and covered with lamp black, the cloth being then dipped in alkali, the manganese precipitates in brown oxide which affords a mark not to be discharged by bleaching, or by the printing processes.

I dissolved in four ounces of water one ounce of the sulphate of Manganese without its water of crystallization; that is to say, it was in the state it possesses when oxigen gas is procured from the black oxide of manganese, by means of the sulphuric acid, and by raising the heat to ignition at the end of the process. This solution was thickened with one dram of fine gum adragan in powder, and coloured with lamp black, in order to distinguish exactly the impression which may be easily made with this black saline metallic mass, of which nevertheless, we cannot make effective use without plunging the end of the marked piece into an alkaline ley, taking care that it shall not first be wetted with water, which would carry off the saline matter. The ley may be made with potash or soda, in the proportion of one part alkali to nine or twelve parts water. It may be used in the state of carbonate, or rendered caustic with half a part of quick lime. The precipitation of the oxide of manganese from the marks by either of these alkaline solutions will take place (exclusive of the stain from the lamp black) of a yellowish white colour, which will become more and more brown by attracting the oxigen of the atmosphere. The change of these marks to the brown, and even to a deeper colour inclining to black, will

will take place very speedily by bleaching with the oxigenated alkaline muriatic ley, the pieces of which the ends have been plunged for a few minutes in the alkaline as before prescribed. These marks of the brown oxide of manganese resist not only all the bleaching materials, and all acids of a requisite force, but likewise the most complicated process of manufacture of calico printing.

Section II.

The acetate of manganese cannot be used without the same manipulation, and as it is more costly it must be rejected.

If the acetic acid had not a much stronger affinity with manganese than it has with iron, and if it disengaged itself as readily from the acetate of manganese as it does from the acetic solution of iron by evaporation and drying, we should be able to procure indeliable marks in the most simple manner, by depositing the oxide of manganese on piece goods by means of the acetic acid, and afterwards simply leaving the oxide to the attraction and saturation of oxigen from the air. The acetic solution of manganese is very readily obtained by mixing a proper quantity of acetate of lead in a solution of sulphate of manganese. But as this acetic solution affords no advantage in marking piece goods beyond those of the sulphate of manganese, and as it requires precisely the same management as that described in the last section, and it is likewise more expensive, it deserves to be rejected.

Section III.

Sulphate of manganese with acetate of iron treated as before. It dries more quickly.

Two ounces of sulphate of magnesia dissolved in eight ounces of the acetic solution of iron, concentrated to the point indicated by twenty degrees, afford when thickened with one fortieth part of gum adraquack, a deep yellow liquor which becomes more and more brown, when treated absolutely in the same manner as described in the first paragraph. The acetic solution does not, however, afford any other advantage but that of causing the marks to dry a little more speedily; for the oxide of iron dissolves in acids accordingly, as it is oxigenated. I give the preference to gum-adragant for thickening colours, to other gums and to starch, because these substances weaken the colours too much, if however, there should be any objection to gum-adragan in coarse goods, starch may be then used.

 Section IV.

Section IV.

Marks printed with the grey oxide of manganese obtained in washing the residual sulphate, afford fixed marks by simple impression.

If care be taken in the process of disengaging oxigen gas from a mixture of the black oxide of manganese and sulphuric acid, not to carry the fire to ignition, the saline residue remains blackish, instead of becoming yellowish white by strong heat. When this residue is dissolved in water, it leaves behind it an oxide of a deep grey, which acquires a very pasty consistence on the filtre. This oxide mixed with a very little water thickened with gum adragant, may be used to print marks of a very deep grey, which dries speedily; and this colour does not wash out with water, even though the subsequent dipping in an alkali be omitted. It is so fixed that it not only supports the action of all acids of the manufacturing strengths, but likewise all the bleaching and printing processes without attracting the colouring matter of any dye whatever.

Section V.

Addition of the nitro muriate of tin to the marking oxide. It affords a dye.

If there were no reason to fear injuring in a slight degree the place where the mark is made, it would be advantageous to employ equal parts of the last described grey paste, and of a nitro muriatic solution of tin, containing one fourth part of the metal, and thickened with gum-adragath. This colour is as unalterable as that of the fourth section; and it has the additional advantage, that its oxide of tin being saturated with oxide of oxigen, attracts the colouring parts of any tincture, and acquires a puce colour by madder. I must observe on this occasion, that by the same madder dye, the colours of marks from the oxide of manganese saturated with oxigen, become of a deep puce colour, inclining to black, whereas in a less oxigenated state they acquire fainter shades. In all these circumstances however, it is requisite, that the quantity of metallic oxide should be as great as possible; otherwise the shades will be various, and less intense.

Section VI.

Experiments with the precipitate of manganese and solution of iron.

As many insoluble metallic oxides acquire the property of adhering to stuffs by means of acid, I did not fail to try whether the same would be the case with the precipitate of manganese saturated with oxigen. For this purpose I dissolved one part

of

of sulphate of manganese in six parts of water, and precipitated the metal by adding to the point of saturation a caustic alkaline ley, made with half a part of quick lime, four parts of water, and one part of calcined potash of the shops. The precipitate was yellowish white. To the whole aqueous mass I then added a sufficient quantity of oxigenated muriatic alkaline ley, until the precipitate was completely saturated with oxigen, and its brown colour became no deeper. I afterwards collected on a filtre the precipitate or brown oxide of manganese, where, by the drainage of its water, it became pasty. This brown paste, mixed with half its weight of the most concentrated acetic acid no longer afforded any but a weak brownish shade; it was the same with a small addition of one or the other of the three ancient mineral acids in a state of solution. I did not succeed better by mixing one part of the same brown paste with an equal quantity of the acetic solution of iron, marking 20° of the areometer of the saltpetre makers and thickened with gum adraganth. This acetic solution of iron containing only the quantity of oxigen necessary for the solution of the metal ceased by a stronger affinity, the excess of oxigen of the brown oxide of manganese, which in its turn became dissolved, and the mixture of the two metallic solutions afforded a yellow reddish very deep and transparent liquid, which confirms the fact that a metal saturated with oxigen requires less acid for its solution than if it were in an opposite state, and that being then furnished with an excess of acid, the solution saturated with oxigen can admit a portion of another metal without becoming turbid. This mixed solution of the two metals afforded me only a rusty yellow which was discharged by weak sulphuric acid completely, in somewhat less time than was required to take out a rust spot in a less oxigenated state. In order to obtain from the mixture of these two metallic solutions an indelible marking colour, it is necessary that the marks should be steeped for several minutes in an oxigenated muriatic alkaline ley, to precipitate and saturate the oxigen of the oxide of manganese. By mixing half a part of the brown paste of manganese to two parts of the solution of the two metals the new portion remains untouched and renders the whole turbid. This turbid mixture left only a light brownish mark on piece goods, which had remained long in the diluted sulphuric acid.

Experiments with the precipitate of manganese and solution of iron.

By means of the muriatic solution of tin, which has the property of taking the oxigen from many vegetable, animal, and mineral substances, and which, on this account, is very useful in dying, as well as in calico-printing. We may discharge and dissolve instantly the deepest oxide of manganese and of iron, which proves the preponderating affinity of tin towards oxigen beyond that of manganese or of iron.

N. B. There is no reason to object to steeping the marked goods in an alkaline ley; the operation is speedily made without sensible loss of potash or of soda, if the operation of lixiviating be immediately proceeded upon, for which the remainder of the ley may be used. And generally, if the practice be used which has been adopted for a number of years, of rendering the alcalies caustic with quick-lime, the saving will be considerable and with better effect.

XIII.

Note on the Formation of Water by mere Compression; with Reflections on the Nature of the Electric Spark. By M. BIOT.*

That oxigen and hidrogen unite by pressure.

IT was some time ago that, in conversation with M. Berthollet on the nature and properties of heat, I communicated to him the persuasion I had, that the combination of hidrogen and oxigen gases might be determined without the aid of electricity, and merely by a very rapid compression. This result appeared to me a consequence so immediately following the observations already made on the heat disengaged from air by compression, that I thought it needless to ascertain it in any other manner. But having since conversed with Mr. La Place, he appeared so interested as strongly to urge me to a verification. I therefore made the experiment, which completely succeeded. It was made in the cabinet of the Polytechnic School. I am greatly indebted to M. Hassenfratz, professor of natural philosophy in that establishment, for the

* Read to the National Institute of France, and inserted in the Annales de Chimie. LIII. 321.

great

great attention he paid in causing the requisite preparations to be made, and for his personal assistance in repeating it.

We took the syringe of an air-gun, the bottom of which was closed by a very thick glass, in order that we might observe the light disengaged as usual by compression. This syringe was of iron; it had a cock on one side to introduce the gases, and its lower extremity on the side of the piston was enveloped by a cylinder of lead, sufficiently weighty to accelerate the fall and render the compression more rapid. This apparatus was first tried by introducing atmospheric air; but though the experiment was made in the dark, no perceptible light was seen, probably because the violent motion necessary for the rapid compression, prevented the operator from looking so directly through the glass as to perceive the transient light which compression disengages, and which I myself had several times seen.

Experiment in proof. The syringe of an air-gun filled up so as to receive the gases and admit of inspection into its chamber, was first tried with common air,

Immediately after this trial a mixture of hidrogen and oxigen gases was introduced into the syringe, and a stroke was given. An extremely brilliant light appeared with a loud detonation: The glass bottom was driven out: The copper screw which retained it in its place was broken; and the person who held the syringe had his hand slightly burned and wounded by the force of the explosion.

and afterwards with a mixture of oxigen and hidrogen. The glass was broken by strong luminous explosion.

The experiment was repeated, by substituting a brass bottom of one entire piece screwed on instead of that of glass, and a new mixture of the gases was introduced. The first stroke of the piston produced an explosion, which was heard like the loud crack of a whip; but a second stroke with a new charge of the gases, caused a detonation which broke or rather tore the body of the syringe with a violent explosion.

Repetition with a metallic cap. The syringe burst.

After these phenomena there can remain no doubt respecting the combination of the two gases; as it is known that this combination produces the detonation by the immense quantity of heat disengaged when they pass to the liquid state; a heat which is sufficient to reduce them immediately into vapour, and give them an excessive dilatation in that state. It was not therefore thought necessary again to repeat this experiment, which is attended with some danger.

The theory of these phenomena is extremely simple. A rapid compression forces the gases to abandon a very great

Theory, the gases give out heat.

 quantity

quantity of heat, which not being capable of immediate dissipation, raises their temperature in the instant sufficiently to inflame them in this state of compression.

Thus it is that we find in the two gases all the elements necessary for their combination, independently of the electric spark or external heat. We might probably in the same manner, and without any foreign agent, produce all the gaseous combinations which require an increase of temperature.

Deduction. The electric spark may consist merely of light driven from compressed air.

This identity of results has led me to a notion which I submit to the judgment of philosophers. It is known, and M. Berthollet has shewn it in his Chemical Statics, that electricity in passing through bodies, produces a true compression of their particles. This effect is produced with the most extreme velocity, as may be proved by an affinity of experiments. Now electricity possessing a velocity so great, it is impossible that it should not disengage light from the air, since we can disengage it by a compression so much less rapid. In this way it is, that we are led to a conclusion, that this result of the electric spark is the purely mechanical effect of compression.

More ample explanation. The extreme velocity of electric matter will strongly compress free air,

If we now compare what passes in our condensing pump and in the eudiometer of Volta, we shall find that the analogy is complete. Only that in the first case we are obliged to confine the air, because the velocity we can give to the piston is limited. Whereas in employing electricity, the particles are compressed by a velocity so great that they can never withdraw themselves with sufficient speed from its effort. Therefore the compression may be equally well made in the open air, together with the disengagement of light or the spark, which is its consequence. But this effect is local; and if the gases be not susceptible of combining together, should after each explosion return to their primitive dimensions, they must immediately resume in this dilation all the heat they had before disengaged, so that there cannot be effected any lasting change in their constitution. This explains why no alteration has ever been seen in very pure unmixed gases, when subjected to the action of the electric spark.

and also the rare fluids in our vacuum.

This light which electricity disengages from the gases by compression, it must also disengage from the more rarified gases, and on account of its extreme velocity, it must disengage it even from vapours, when experiments are made under the receiver

receiver of an air-pump or in the torricelian vacuum: For we can never form a perfect vacuum with our machines, and even in the tube of the barometer mercury always exists in the form of vapours. These vapours, though very rare, still contain a large quantity of caloric, which the electricity must disengage in its passage by compression; but the instantaneous augmentation of electricity which results, cannot become sensible on account of the little density of the medium; but this increase is perceivable in denser air, as we see in the instrument called Kinnersley's thermometer.

Conclusion.

The considerations which I have here made, appear to me to point out with some probability, that the phenomenon called the electric spark, is owing to the light disengaged from the air by compression during the passage of the electricity; so that this phenomenon is purely mechanical, and not at all electric in itself. This is the notion which I submit to the judgment of philosophers: if it be true, it must tend considerably to diminish the number of hypotheses which have already been made, or may be made on the nature of electricity. For this reason it is that I have offered it to their consideration, requesting that it may not be be thought that I consider it as of greater importance than their deliberate examination may bestow upon it.

XIV.

Account of Thermometers for registering the highest and lowest Temperatures in the Absence of the Observer. By F. A.

To Mr. NICHOLSON,

SIR,

Thermometers and barometers for registering the weather.

MANY contrivances have been proposed and adopted for registering all the stations of the thermometer and barometer, by means of a float or other equivalent instrument carrying a pencil, which marks its situation on a surface gradually moved along by means of a clock. These, of which meteorologists know the value, are nevertheless expensive, and require a degree of care and management sufficient to render simpler contrivances

Six's thermometer.

contrivances acceptable. Mr. James Six communicated, about 25 years ago, to the Royal Society a thermometer, in which two small indicating pieces were driven by the fluid in the tubes to stations where they stuck, and remained after the change of temperature, and shewed the highest and lowest degrees that had occurred since the last placing of them in contact with it. As this instrument is sufficiently known, and I am now to advert to a simpler contrivance, I will dismiss that subject and advert to this last.

Objections to it.

In Mr. Six's complicated thermometer the tubes were vertical, and the indexes stuck in the glass by their spring; besides which, a small piece of steel wire being exposed to alcohol, was at length oxided and set fast. The other contrivance now to be seen in all our London shops, and respecting which you will do an acceptable service to your readers and the scientific world, by inserting a sketch in your Journal, consists simply in two thermometers, one mercurial and the other of alcohol (*Fig.* 1, *Pl.* X.) having their stems horizontal; and the former has for its index a small bit of magnetical steel wire, and the latter a minute thread of glass, having its two ends formed into small knobs by fusion in the flame of a candle.

Another thermometer, which shews the greatest and least temperatures.

Description. It has a mercurial thermometer which shews the maximum; and a spirit thermometer for the minimum.

The magnetical bit of wire lies in the vacant space of the mercurial thermometer, and is pushed forward by the mercury whenever the temperature rises and pushes that fluid against it: but when the temperature falls and the fluid retires, this index is left behind, and consequently shews the maximum. The other index, or bit of glass, lies in the tube of the spirit thermometer immersed in the alcohol, and when the spirit retires by depression of temperature, the index is carried along with it in apparent contact with its interior surface: but on increase of temperature the spirit goes forward and leaves the index, which therefore shews the minimum of temperature since it was set. As these indexes merely lie in the tubes, their resistance to motion is altogether inconsiderable. The steel index is brought to the mercury by applying a magnet on the outside of the tube, and the other is duly placed at the end of the column of alcohol by inclining the whole instrument.

Question. Why the small glass index always remains in the spirit.

I beg you will explain the motion of the glass index. I can easily understand from the general fact *that mercury repels steel*, that this fluid will drive the steel index before it; but I cannot make

make out to my satisfaction, how the spirit, by attracting glass, can prevent the other index from ever rising out of its surface. Perhaps this thing may be already explained in elementary books; but whether it be or no, I am sure that an account in your clear and popular way cannot be thought superfluous.

I am respectfully,

Sir,

Your obliged

F. A.

REPLY.—W. N.

Explanation. If the small glass piece were protruded beyond the spirit, the fluid would hang to it and draw it back.

When the surface of the column of spirit is viewed by a magnifier, it is seen to have the form of a concave hemisphere, which shews that the liquid is attracted by the glass. The glass in that place is consequently attracted in the opposite direction by a force equal to that which is so employed in maintaining that concave figure; and if it were at liberty to move, it would be drawn back till the flat surface was restored. Let us suppose a small stick or piece of glass to be loose within the tube, and to protrude into the vacant space beyond the surface of the alcohol. The fluid will be attracted also by this glass, and form a concave between its surface and that of the bore of the tube. But the small interior piece being quite at liberty to move, will be drawn towards the spirit so long as the attractive force possesses any activity; that is, so long as any additional fluid hangs round the glass; or in other words, until the end of the stick of glass is even with the surface. Whence it is seen that the small piece of glass will be resisted, in any action that may tend to protrude it beyond the surface of the fluid; and if this resistance be greater than the force required to slide it along in the tube (as in fact it is), the piece must be slided along as the alcohol contracts; so as always to keep the piece within the fluid. And this fact is accordingly observed to take place.

Abstract

XV.

Abstract of a Memoir on Milk. By M. THENARD.[*]

Component parts of milk.

IN a memoir which I read to the Philomatic Society in Praireal last, I shewed that milk always contains the free acetous acid in a greater or less quantity. At the same period Messrs. Fourcroy and Vauquelin found that it also contains phosphate of magnesia, and that the lactic acid of Scheele, or that which is obtained from serum of milk spontaneously coagulated, is merely the acid of vinegar combined with an animal matter. So that in the present state of our knowledge we must consider milk as composed of, 1. Water; 2. Acetous acid; 3. Caseous matter; 4. Butteraceous matter; 5. Sugar of Milk; 6. Extractive matter; 7. Muriate of soda and of potash; 8. Sulphate of potash; 9. Phosphate of lime; 10. Phosphate of magnesia.

Of these eleven substances there is one which I particularly examined some months ago, namely cream. I was desirous of ascertaining the circumstances which govern its separation, and particularly its transformation into butter.

The separation of cream and of butter does not require access of air.

I had before observed that milk coagulates as readily in closed as in open vessels; I know that no gas is disengaged in this decomposition, and that, in order to effect it with rapidity, it is needful only to raise the temperature to between 20° and 40° (Reaumur I suppose; and, if so, answering to 77° and 122° Fahrenheit). It was clear, therefore, that the air contributes neither to the formation nor the separation of cream, but that it exists ready formed in milk; but it remained to be shewn what are the principles which enter into its composition. Being persuaded, from various observations I had made, that it is only an intimate mixture of butter, cheese, and serum, I proceeded to ascertain this point by mixing a pint bottle (English quart) of recent cream nearly to its neck, from which I displaced the remaining air by carbonic acid. I then closed it well, and agitated it strongly in every direction for half an hour; at the end of which time the contents having

[*] Soc. Philomath, No. 96.

become

become very thick and adhering strongly to the sides of the bottle, gradually became detached, and soon afterwards were converted into a white liquid, in the midst of which swam a yellow mass of excellent butter. Hence it follows, that the butter exists in the milk, and is separated when the milk, being deprived of the vital action, is left to itself. At this time, either by the formation of an acid arising doubtless from a decomposition of the extractive matter, or perhaps from the less specific gravity of the butter compared with that of the cheese; for the butter begins to separate almost at the moment that milk is poured into a vessel;—the milk is decomposed, the cream rises to the top, and from this last, by agitation, and more particularly by the assistance of a temperature between 15° and 20° (66° to 77° F.), butter is obtained together with butter-milk, which is a white very mild liquor, in which some butter and cheese are suspended in a very divided state. But the butter thus obtained is not pure: It still contains a portion of cheese amounting sometimes to the sixth part of its weight; and this is the cause of its speedily becoming rancid, particularly in summer. When the cheesy matter is separated by fusion, the butter may be kept a long time. It is true indeed, that by this fusion it acquires an acridness which greatly limits its uses, and makes it unfit to be employed in frying; but this disadvantage might be remedied by keeping the temperature much lower than is usual. Clouet first made this observation; and hence the following process may be adopted for purifying butter, or separating the cheesy matter without giving it a bad taste.

Process of butter making.

1\. Let the butter be melted on the water bath, or at a degree of heat not exceeding the 66° of Reaumur. 2. Keep it melted till all the cheesy matter is collected in white flakes at the bottom of the vessel, and the melted butter is transparent. 3. At this period decant it, or pass it through a cloth. 4. Let it be cooled in a mixture of equal parts of pounded ice and sea-salt; or if ice cannot be procured, then in cold spring-water, making use of broad shallow vessels. Without this precaution the butter would become lumpy by crystallizing, in which state it could not be served at table. Besides which, the parts being condensed by this sudden cold, are found to resist the action of the air more effectually. With this last intention it is also proper to

Purification of butter by fusion, which separates the cheesy part.

to cover the pot in which the butter is kept very exactly, and to place it in a cold exposure, such as a cellar. By this treatment butter may be kept for six months or more, and will be nearly as good as fresh butter, particularly after the top is taken off. It is even possible to give this fused butter to a certain point the appearance of fresh butter, by beating it with one sixth part of its weight of the cheesy matter; and so likewise rancid butter may be considerably amended by the process of fusion and cooling here prescribed.

SCIENTIFIC NEWS.

Temperature of the Sea.

General facts respecting the temperature of the sea.

MR. PIRON has lately communicated to the French National Institute a memoir on the temperature of the sea; an interesting subject, capable of being applied to various useful purposes, and which has accordingly engaged the attention of a considerable number of philosophical observers. His general facts are, 1. The mean temperature of the sea at its surface is commonly more elevated than that of the air. 2. It is higher the nearer to the continents and large islands. 3. At a distance from the shore in deep seas the water is colder below than at its surface; and the more the greater the depth. All the observations seem to shew, that in the abysses of the ocean, as well as on the summits of mountains, even under the equator, eternal frost prevails. 4. A similar cold is observed in extensive lakes, and even within the earth at great depths, but it appears to be less sudden. 5. These results concur in proving, that the temperature within the earth is not every where the same and equal to $93\frac{1}{4}^{\circ}$, as has been long thought (about 50° Fahr. whether this be centigrade or Reaumur's scale.)

At great depths it is eternally frozen.

Spent Oil of the Curriers.

Concerning the oil and compounds used in currying leather.

The process by which the curriers impregnate their skins is by smearing the oil upon the wet skin, into which it penetrates as the moisture evaporates. A pure oil could not perhaps be

be thus spread, and most probably would not enter the skin with the desired effect, or render it as supple as that oil which from experience they are led to prefer.

The celebrated Seguin has directed his attention to this ingredient of such extensive manufactoring utility. He remarks, that this material (by the name of *Degras)* is of two kinds in France; *viz.* the common sort and that of Niort. The first is the immediate product of the chamoying of skins, which are cleared of their surplus oil by solution of potash. It therefore contains not only soap, but likewise gelatine. It is evaporated to dryness and then sold as *Degras*. At Niort it is decomposed by sulphuric acid, and the precipitate is called the *Degras* of that town.

Mr. Seguin finds by analysis, that this last is oxigenated oil, whereas the other is a compound of soap and gelatine. He succeeded in giving to whale oil all the properties of the *Degras* of Niort, by boiling one pound for a few minutes with half an ounce of nitric acid at 25 degrees. He observed that no gas is disengaged in this operation; but that water and nitrate of ammonia are formed; and he concludes that the oil was oxigenated, not by absorbing the oxigen of the acid, but by yielding to it part of the hidrogen which was one of its own component parts. The result is the more interesting, as the *Degras* of Niort being much more esteemed than the common sort, the curriers may hereafter, instead of paying a great price for it, make it in as large quantities as they please by following the process here indicated.

Note respecting the Decomposition of Sulphate of Lead by the Muriatic Acid. By M. Descotils *.

If the sulphate of lead be treated with muriatic acid rather concentrated, that metallic salt is totally dissolved, provided the proportion of acid be rather in excess. This solution requires heat to effect it. Upon cooling, the muriate of lead crystallizes in great quantity; and it is much more speedily obtained by the addition of a small quantity of cold water. If the supernatant fluid be separated from the crystallized salt, a pre-

Sulphate of lead is soluble by heat in muriatic acid; Muriate of lead separates by cooling. This is soluble in water, and may be again decomposed by sulph. acid.

* Soc. Philom. No. 96.

a precipitate is obtained from the former by muriate of barytes. The muriate of lead is soluble in water, and may be almost entirely decomposed by sulphuric acid, which forms sulphate of lead.

Another instance in the analysis of antimonial galena.

This fact deserves to be carefully examined with relation to the play of affinities; and it may be of importance in the analysis of mineral and metallic substances. In fact, if an alloy contain a small quantity of lead, and it were necessary, in order to dissolve the alloy, to employ the nitro-muriatic acid, it would be very possible, and I have found it so, that sulphuric acid would not indicate the presence of lead. The following is another instance: If an antimonial galena be treated with nitric acid and sulphate of lead thus formed, this last would be decomposed by the muriatic oil (or acid?) which might be employed to take up the oxide of antimony, and the muriate of lead would remain dissolved after the addition of water. If care were not taken to examine the filtered liquor, a loss would be experienced which it would be difficult to account for.

Extract of a Letter from Naples, dated August 13.

Account of the late eruption of Mount Vesuvius.

"Yesterday at ten o'clock at night, the eruption of Vesuvius, of which the earthquake seemed to be the forerunner, took place. We were going to visit the crater when the cries of the people and a volume of flame informed us that the volcano had opened. The lava precipitated itself in three seconds from the last peak of the mountain and took a direction towards the valley, situated between Torre del Greco and Torre del l'Annunziata, two towns on the sea coast, beyond Portici, and seven or eight miles from Naples.

We set off immediately to see this wonderful and tremendous phenomenon nearer. From the place of our departure, we saw the whole course of the lava, which extended nearly two miles, from the crater to the houses that join the two towns. The sight was the most magnificently frightful that could be seen. I contemplated the cascades of flame pouring from the top of the mountain, and shuddered at seeing an immense torrent of fire ravage the finest fields, overthrow houses, and destroy in a few minutes the hopes and resources of a hundred families.

A line

A line of fire marked the profile of the mountain: a cloud of smoke, which seemed to send forth from time to time flashes of lightning, hung over the scene, and the moon appeared to be pale: Nothing can adequately describe the grandeur of the scene, or give an accurate idea of the horror of it. As we approached the spot ravaged by this river of hell, ruined inhabitants having quitted their houses—desolated families trying to save their furniture and provisions, last and feeble resource—an immense croud of curious spectators, retreating step by step from the advancing lava, and testifying by extraordinary cries their wonder, fear, and pity—the frightful bellowing of the mountain, the frequent explosions which burst from the bottom of the torrent, the crackling of the trees devoured by the flames, the noise of the walls falling, and the lugubrious sound of a bell, which the religious of the Camaldules, isolated on a little hill and surrounded by two torrents of fire, rang in their distress. Such are the details of the frightful scene to which I was witness.

Account of the late eruption of Mount Vesuvius.

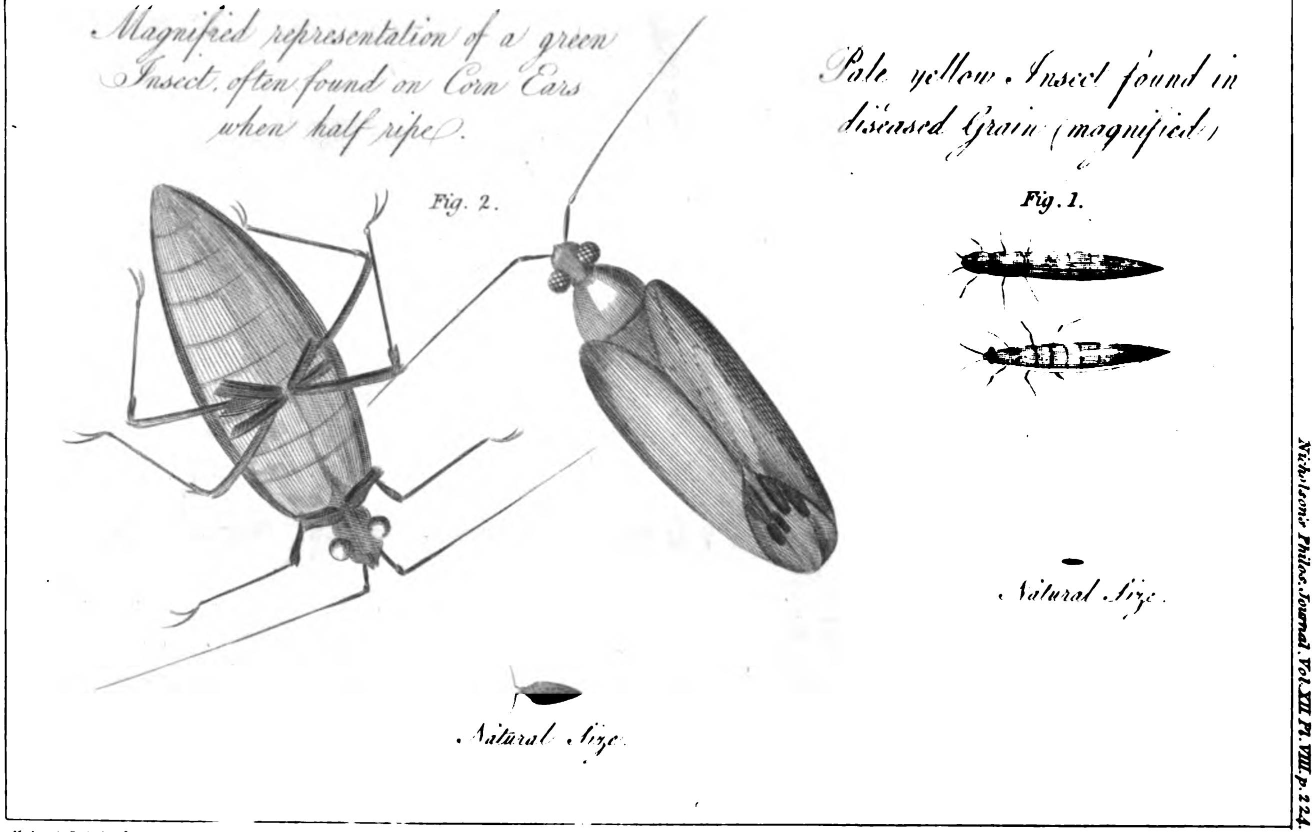
The moment we arrived the lava was crossing the great road below Torre del Greco. To see it better we got into a beautiful house on the road side—from the terrace we saw the fire at no more than fifteen paces from us—in a minute we descended, and twenty minutes afterwards there remained of the house but three large walls. I approached as near as the heat and flow of the current would permit me, I attempted at different times to burn the end of my handkerchief in it—I could only do it by tying it to my cane. The lava does not run in liquid waves; it resembles an immense quantity of coals on fire, which an invincible strength had heaped up and pushed on with violence. When it met with a wall, it collected to the height of seven or ten feet, burnt it, and overthrew it at once. I saw some walls get red hot, like iron, and melt, if I may use the expression, into the lava. In its greatest speed and on an horizontal road, I reckoned that the torrent travelled at the rate of eighteen inches a minute. Its smell resembled that of iron red hot.

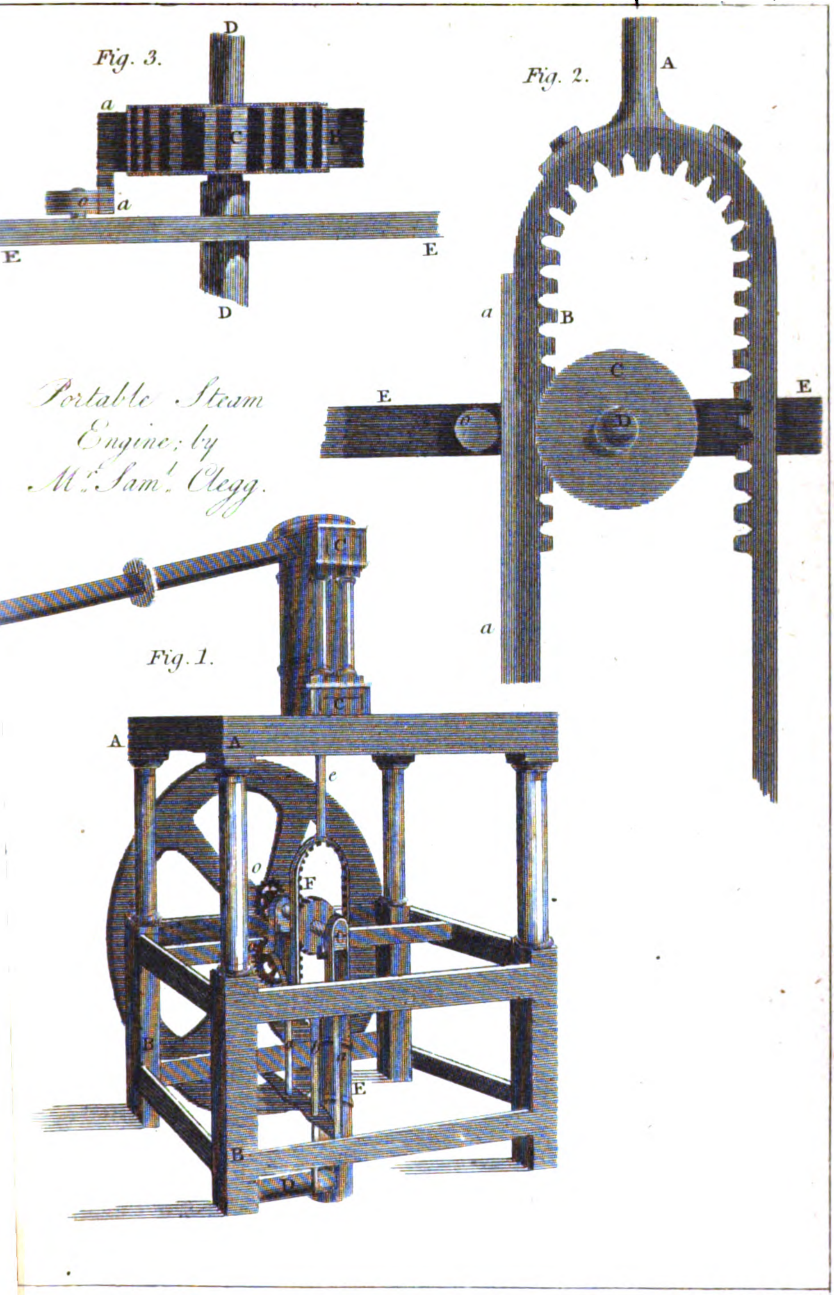
Morning Chronicle.

Applicative Compass for taking Bearings on a Chart, by N. D. Starck, *Esq. of the Royal Navy.*

Compass for taking bearings, on a chart.

This instrument, seen in *Fig.* 6, *Plate* XI. consists of an inner and outer brass concentric circle; the latter of which, when in use, is to be applied to a chart, so that its cardinal points may agree with those of the draft, and its central (metallic) point be directly over the ship's place. The inner circle is to be set to the variation; and the thread from the center being laid, will shew either bearings by compass, or true bearings, according to the circle upon which they are read. It is obvious also, that the instrument may be used in delineating, plotting, and for various other useful purposes.

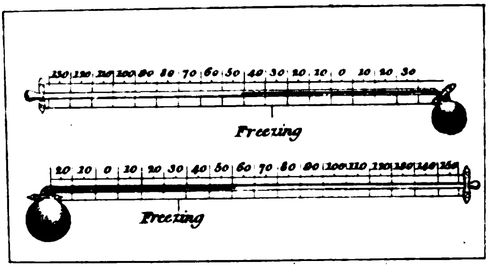

Magnified representation of a green Insect, often found on Corn Ears when half ripe.
Fig. 2.
Natural Size.
Pale yellow Insect found in diseased Grain (magnified)
Fig. 1.
Natural Size.



Mutlow Sc. Russell Ct.

Self registering Thermometers.

Fig. 1.



Plan of Mr. Bloore's Truss, by which the Roof of Clapham Church was raised.

Fig. 2.

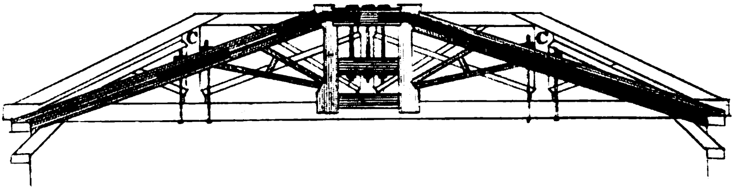
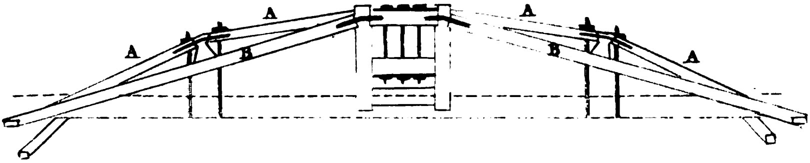


Fig. 3.



Mutlow Sc. Russel Ct.

Improved Sheep-fold
by T. Plowman Esq.r

Fig. 1.

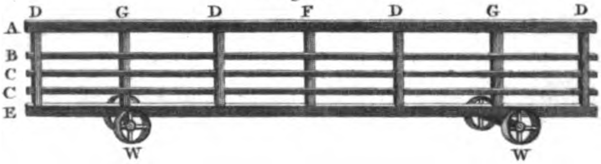


Fig. 3.

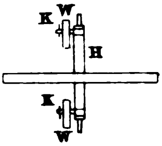


Fig. 2.

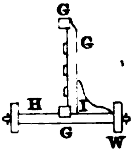
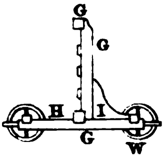
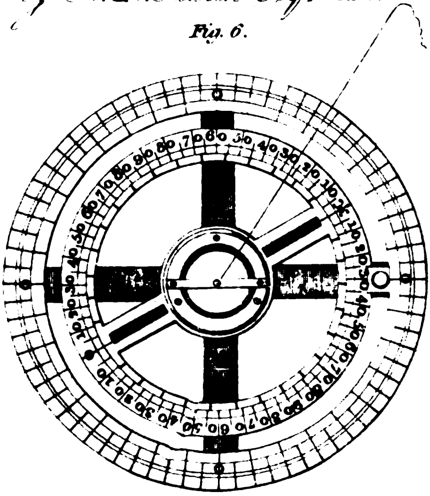


Fig. 4.



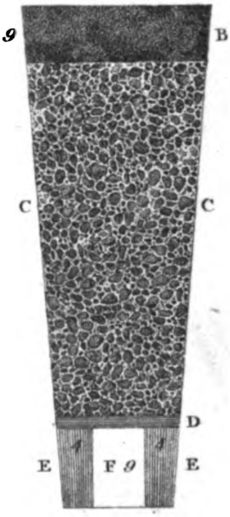
Applicative Compass
by N. D. Starck Esq.r R. N.

Fig. 6.



Drain, by
J. C. Curwen Esq.r

Fig. 5.



Mutlow sc. Russell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1805.

ARTICLE I.

On the Division of an Arch of a Circle into two such Parts, that their Sines, or Cosines, or Versed-Sines, shall have a given Relation. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR,

BEING at present on a visit to my friend Michael Fryer, teacher of the mathematics at this place, I have availed myself of the opportunity to consult his very extensive mathematical library, with a view to discover how far the following theorems and problems are original; thinking it possible, at least, that similar propositions might be met with in the works of the early geometricians, particularly in the tracts on Angular Sections, by Vieta, Oughtred, Wallis, and others, which I had never before been able to meet with; but I have found only one of them to have been already treated, of which notice shall be taken in its proper place: nevertheless, it is not improbable but that similar theorems and problems are scattered up and down in the different works on geometry at present in existence: As this essay, however, may claim the merit of ex- Introductory letter.

exhibiting them in one view, and, which is equally desirable, of deriving them from a general principle, I have ventured to offer it for insertion in your Journal.

JOHN GOUGH.

Reeth, near Richmond, Yorkshire,
August 28.

PROPOSITION I. THEOREM.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Let A F be the arch of a circle, (*See Fig.* 1, *Pl.* XII.) A P a tangent at A; F P a perpendicular to A P, then A P is equal to the sine of A F; F P, the part of the perpendicular intercepted by the tangent and the point F in the arch, is equal to its versed sine; and the same line, P M, intercepted again by the circle in M, is equal to the versed sine of its supplement.

Demonstration.

Draw the diameter A K, and the sine F S perpendicular thereto; also from the center O, draw O L at right angles to P M; then, since P A touches the circle in A, P A K is a right angle, (Euc. 16. iij.) Also, the angles F P A, A S F, are right, by construction; therefore A S F P is a parallelogram, the opposite sides of which are equal, namely, A P = the sine S F, and P F = the versed sine A S, (Euc. 34. i.)

Again, since O L is perpendicular to P M, it is parallel to A P and S F, therefore P L = A O, or O K; and F L = S O, (Euc. 34. I.)—But F L = L M, (Euc. 3. III.) Consequently P M = S K, or the versed sine of the supplement A F. Q. E. D.

PROPOSITION II. THEOREM.

If A F B be an arch of a circle, (*See Fig.* 2.) and A P, B R, be tangents at A and B, from any point, F, in the circumference, draw F P, F R, perpendicular to the two tangents, and F Q also perpendicular to the chord A B, then will the rectangle $PF \times FR = \overline{FQ}^2$; and the rectangles $AP \times BR$, and $AQ \times QB$, will also be equal.

Demonstration.

Join A F, F B, and the triangles P F A, Q F B, are equiangular, because they are right-angled at P and Q, by construction; and the angles P A F, Q B F, are equal, (Euc. 32. III.)

Therefore,

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Therefore, as $AF : FB :: PF : FQ$.

Also, the triangles QFA, RFB, are equiangular, for the same reasons.

Therefore, as $AF : FB :: FQ : FR$.

Consequently, as $PF : FQ :: FQ : FR$, (Euc. 11. V.)

And $PF \times FR = \overline{FQ}^2$, (Euc. 14. VI.)

Q. E. 1° D.

Again, by the same triangles, as $FA : FB :: AP : BQ$,

and as $FA : FB :: AQ : BR$;

hence, as $AP : BQ :: AQ : BR$,

Whence $AP \times BR = BQ \times AQ$:

Q. E. 2° D.

Corol. 1. Produce the perpendicular FQ till it meets the circumference again in G, and $PA \times RB = FQ \times QG$:

For $PA \times RB = AQ \times QB$ by the proposition; but $AQ \times QB = FQ \times QG$, (Euc. 35. III.)

Corol. 2. If the lines PF, RF, meet the circle again in M and N, then will $PM \times RN = \overline{QG}^2$:

For $\overline{AP}^2 = FP \times PM$, and $\overline{BR}^2 = FR \times RN$, (by Euc. 36. III.)

Therefore, as $\overline{AP}^2 : FP \times PM :: FR \times RN : \overline{BR}^2$:

But $\overline{AP}^2 : FQ \times QG :: FQ \times QG : \overline{BR}^2$, by Corol. 1.

And $PF : FQ :: FQ : FR$, by the proposition.

Therefore,

$PF \times PM : FQ \times PM :: FQ \times RN : FR \times RN$.

Hence,

$FQ \times QG : FQ \times PM :: FQ \times RN : FQ \times QG$.

Consequently, $PM \times RN = \overline{QG}^2$.

Corol. 3. Draw the diameters AK, BD, and make FS, FT perpendicular to AK, BD; then $AK \times BT$ (the rectangle of the versed sines) $= \overline{FQ}^2$; $SF \times FT$ (the rectangle of the sines) $= AQ \times QB$; and $SK \times TD$ (the rectangle of the supplementary versed sines) $= \overline{QG}^2$. These things follow from Props. I. and II.

Proposition III. Problem.

To divide a given arch of a circle (AB) into two parts (AF, FB), so that the rectangle of their versed sines (AS, BT) may be equal to a given magnitude, or square, ($m \times m$).

 Construction.

Construction.

Division of an arch of a circle into two parts, having their fines, or cofines, or v. fines, in a given ratio.

From any point, Y, in the right line A B, draw Y W at right angles to the fame, making it equal to the given right line m; through W, parallel to Y B, draw W F, and let it cut the arch A B in F, then will A F, F B, be the required arches.

Demonftration.

Draw F Q perpendicular to Y B, then $FQ^2 = WY^2 = m \times m$; by Conft. and Euc. 34. I.; but the rectangle of the verfed fines of A F and $FB = \overline{FQ}|^2$, (by Cor. 3. Prop. II.); therefore this rectangle is equal to $m \times m$, the given fquare. Q. E. D.

PROPOSITION IV. PROBLEM.

To divide A F B, a given arch of a circle, (*See Fig.* 3.) into two parts, A F, F B, fo that the rectangle of their fines may be equal to a given fquare, ($n \times n$)?

Conftruction.

To make the conftruction general, let A F B be greater than a femicircle, join A B, and in it take Q, making $AQ \times QB = n \times n$; alfo in A B produced take q, fo as to make $Aq \times qB = n \times n$; draw Q F, qfg, perpendicular to A B; then will A F, F B, or A g, g B, or A f, f B, be the required arches.

Demonftration.

This is evident from Cor. 3. Prop. II. and the conftruction.

To find *the limits*, bifect A B in Z, draw alfo the radius O N parallel to Z B, and make N E perpendicular to A B produced; then, if $n \times n$ be greater than $AZ \times ZB$, F is an imaginary point, becaufe $AQ \times QB$ cannot exceed $AZ \times ZB$, by Euc. 5. II. Again, if $n \times n$ be greater than $AE \times EB$, the points f, g, are imaginary, becaufe $Aq \times qB$ cannot exceed $AE \times EB$, feeing E N touches the circle in N, and is parallel to qg: Thefe things being premifed, it will be eafily perceived, that when A F B is lefs than a femicircle, it can only be divided in one point to anfwer the conditions of the queftion, becaufe the point N will be in the oppofite fegment; but when it exceeds a femicircle, it will admit of being divided into one, two, or three points, according to circumftances, or even the conftruction may prove impoffible. Q. E. D.

Scholium

Scholium.

This problem is conſtructed at page 342 of the Appendix to Simpſon's Algebra, 2d Edition; and at page 140 of his Select Exerciſes, 1ſt Edition; but the conſtructions given by that able geometrician do not ſhew the various limits of the queſtion with that degree of perſpicuity which appears in the preſent method.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines or v. ſines, in a given ratio.

Lemma.

Let A B C D be a ſquare, (*See Fig.* 4.) from any two adjacent ſides of which, C B, C D, take the ſegments T C, C S, then will the rectangle of the remaining ſegments $BT \times SD = \overline{BC}|^2 + TC \times CS - BC \times CT - BC \times CS$.

Demonſtration.

Draw S G, T H, parallel to B C, C D, and let them interſect in F;—

Then the rectangle $FTCS = TC \times CS$,

and the rectangle $FHAG = BT \times SD$,

But $FHAG + GBCS + HFSD =$ the ſquare A B C D; (Euc. 1. II.)

Add F T C S to both,

Then $FHAG + GBCS + TCDH = ABCD + FTCS$;

But C D is equal to B C,

Therefore $FHAG = ABCD + FTCS - BC \times CT - BC \times CS$;

That is, $BT \times SD = \overline{BC}|^2 + TC \times CS - BC \times CT - BC \times CS$. Q. E. D.

Proposition V. Problem.

To divide A F B, a given arch of a circle, (*See Fig.* 5.) into two parts, A F, and F B; ſo that the rectangle of their coſines may be equal to a given ſquare, $k \times k$?

Conſtruction.

Join A B, and from the center, O, draw O Z perpendicular to A B; in Z O take Z V equal to the given line, k, and join B V; draw the diameter, H h, parallel to A B, and divide it in I ſo as to make $HI \times Ih = \overline{BV}|^2$; from I draw I Q perpendicular to A B, and when produced let it meet the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Demonstration.

Division of an arch of a circle into two parts, having their sines, or cosines, or versed sines, in a given ratio.

Let F I meet the circle again in G, draw the diameters A K, B D, and the sines F S, F T;

Then the cosine $OS = OA - AS$,

and the cosine $OT = OB - BT = OA - BT$;

Hence $SO \times OT = \overline{AO}^2 + AS \times BT - AO \times AS - AO \times BT$;

But $AS \times BT = \overline{FQ}^2$, by Prop. II.

Therefore,

$$SO \times OT = \overline{AO}^2 + \overline{FQ}^2 - AO \times AS - AO \times BT.$$

Again, $KS = 2\,AO - AS$,

and $DT = 2\,AO - BT$;

Hence, $KS \times DT = 4\,\overline{AO}^2 + AS \times BT - 2\,AO \times AS - 2\,AO \times BT$;

But $KS \times DT = \overline{QG}^2$, by Cor. 3. Prop. II.

Therefore,

$$4\,\overline{AO}^2 + \overline{FQ}^2 - 2\,AO \times AS - 2\,AO \times BT = \overline{QG}^2;$$

$$\text{Hence, } AO \times AS + AO \times BT = 2\,\overline{AO}^2 + \frac{\overline{FQ}^2}{2} - \frac{\overline{QG}^2}{2}:$$

$$\text{But } SO \times OT = AO^2 + FQ^2 - AO \times AS - AO \times BT = \frac{FQ^2}{2} + \frac{QG^2}{2} - \overline{AO}^2.$$

$$FQ = FI - IQ = BP - OZ,$$

$$\text{and } GQ = GI + IQ = BV + OZ;$$

$$\text{Hence } \frac{FQ^2}{2} + \frac{GQ^2}{2} = \overline{BV}^2 + \overline{OZ}^2;$$

Consequently, $SO \times OT = \overline{BV}^2 + \overline{OZ}^2 - \overline{AO}^2$;

But $\overline{AO}^2 - \overline{OZ}^2 = \overline{BO}^2 - \overline{OZ}^2 = \overline{BZ}^2$;

Therefore $SO \times OT = \overline{BV}^2 - \overline{BZ}^2 = \overline{VZ}^2$, (Euc. 47. I.) $= k \times k$, by construction. Q. E. D.

Limitation.—If Z V be greater than Z O, B V will be greater than B O; *i. e.* F G will be greater than H *h*, which is impossible, Euc. 15. III. therefore Z V, *or k*, cannot exceed Z O.

Proposition VI. Problem.

To divide A F B, a given arch of a circle, (*See Fig.* 6.) into two parts, so that the sum of their versed sines may be equal to a given right line, *u*?

Construction.

Construction.

Draw the radius A O, and the tangent B E; in A O take A I equal to the given line, u; and making I V perpendicular to A O, let it meet B E in V; draw F V to bisect the angle E V I, and let it cut the given arch in F; then will A F, F B, be the required arches.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Demonstration.

Draw the tangent A G, which is parallel to I V, also make F P, F R perpendicular to A G, B E, and let P F produced meet I V in H.

Then since A G, I V, are parallels, and the angle H P A is right, F H V is also a right angle, (by Euc. 29. I.) therefore it is equal to the angle F R V, (by construction.)

But the angles R V F, H V F, are also equal, (by construction); consequently the triangles R F V, H F V, are equiangular; and they have one side common, namely the side V F; therefore F R = F H, (Euc. 4. VI.) and P F + F R = P H = A I, (Euc. 34. I.) = u, (by construction.)

But the sum of the versed sines of A F, F B, is equal to P F + F R, (by Prop. I.) therefore this sum is equal to the given line, u. Q. E. D.

Limitation.—If A I be greater than the versed sine of the whole arch A B, the point F will evidently fall in the opposite segment, and the construction will be impossible.

Again, since the angle I V E is equal to the angle A O B, draw the radius O C, to bisect the angle A O B, and it will evidently be perpendicular to V F; therefore L C, a tangent at C, will be parallel to V F; consequently if A I be so taken, that V may lie in B L produced, the construction will also be impossible; which will therefore happen when u is less than twice the versed sine of the arch A C, or B C.

Corol. Since the sum of the versed sines of two arches is the same with the difference of the diameter and the sum of the cosines, if the latter sum be given the problem may be constructed by the last proposition.

Proposition VII. Problem.

To divide A F B, a given arch of a circle, (*Fig.* 7.) into two parts, so that the sum of their sines may be equal to a given right line, w?

Construction.

Construction.

Division of an arch of a circle into two parts, having their sines, or cosines, or v. sines, in a given ratio.

Draw the radii A O, O B, and the tangents A G, B E, in which take A S, B T, each equal to the half of w; draw S N, T N, parallel to A O, O B; and through their intersection, N, draw N F, parallel to S T, to meet the arch in F, then A F, F B, are the parts required.

Demonstration.

Draw F K, F M, parallel to N S, N T, and let them meet S T in K, M, and A G, B E, in P, R; then it is easily proved that the triangles K F M, S N T, are equal and similar, and that K M = S T; consequently S K = T M.

But the angles K P S, M R T, are right, being equal to the angles O A G, O B E, by construction; and the angles K S P, M T R, are equal; therefore the triangles P S K, R T M, are equiangular, they are therefore equal, (Euc. 4. VI.) because S K = T M; consequently S P = R T; therefore A P + B R = A S + B T = w.

But the sum of the sines of A F, F B = A P + B R; this sum is therefore equal to w.

Limitation.—Join A B, which will be parallel to S F, also let the radius O C bisect the angle A O B, when properly produced, or not, it will pass through the point N. Now if N be in O C produced, N F, being parallel to S T, or A B, will not meet the circle; on the other hand, if N lie between O and A B, F will be in the opposite segment of the circle, consequently the construction is impossible, unless N fall between C and the line A B, or in the versed sine of half the given arch: These things being premised, it will be easily perceived that the sine of the arch A F B is the less limit of the problem, and twice the sine of A C its greater limit.

Concerning

II.

Concerning the State in which the true Sap of Trees is deposited during Winter. By Thomas Andrew Knight, *Esq**.

It is well known that the fluid, generally called the ſap in trees, aſcends in the ſpring and ſummer from their roots, and that in the autumn and winter it is not, in any conſiderable quantity, found in them; and I have obſerved in a former paper, that this fluid riſes wholly through the alburnum, or ſap-wood. But Du Hamel and ſubſequent naturaliſts have proved, that trees contain another kind of ſap, which they have called the true, or peculiar juice, or ſap of the plant. Whence this fluid originates does not appear to have been agreed by naturaliſts; but I have offered ſome facts to prove that it is generated by the leaf †; and that it differs from the common aqueous ſap owing to changes it has undergone in its circulation through that organ: and I have contended that from this fluid (which Du Hamel has called the *ſuc propre,* and which I will call the true ſap,) the whole ſubſtance, which is annually added to the tree, is derived. I ſhall endeavour in the preſent paper to prove that this fluid, in an inſpiſſated ſtate, or ſome concrete matter depoſited by it, exiſts during the winter in the alburnum, and that from this fluid, or ſubſtance, diſſolved in the aſcending aqueous ſap, is derived the matter which enters into the compoſition of the new leaves in the ſpring, and thus furniſhes thoſe organs, which were not wanted during the winter, but which are eſſential to the further progreſs of vegetation.

The common ſap riſes in ſpring and ſummer, but not in winter.

True or peculiar ſap

exiſts in the alburnum during winter: its ſubſequent deſtination pointed out.

Few perſons at all converſant with timber are ignorant, that the alburnum, or ſap-wood of trees, which are felled in the autumn or winter, is much ſuperior in quality to that of other trees of the ſame ſpecies, which are ſuffered to ſtand till the ſpring, or ſummer: it is at once more firm and tenacious in its texture, and more durable. This ſuperiority in winter-felled wood has been generally attributed to the abſence of the ſap at that ſeaſon; but the appearance and qualities of the

That winter or autumn felled wood has its alburnum more firm, &c.

commonly attributed to the abſence of the ſap:

* See Phil. Tranſ. of 1801, page 336.

† Philoſ. Tranſ. 1805, p. 88.

wood

wood ſeem more juſtly to warrant the concluſion, that ſome ſubſtance has been added to, inſtead of taken from it, and many circumſtances induced me to ſuſpect that this ſubſtance is generated, and depoſited within it, in the preceding ſummer and autumn.

—but probably to its preſence.

Du Hamel has remarked, and is evidently puzzled with the circumſtance, that trees perſpire more in the month of Auguſt, when the leaves are full grown, and when the annual ſhoots have ceaſed to elongate, than at any earlier period; and we cannot ſuppoſe the powers of vegetation to be thus actively employed, but in the execution of ſome very important operation. Bulbous and tuberous roots are almoſt wholly generated after the leaves and ſtems of the plants, to which they belong, have attained their full growth; and I have conſtantly found, in my practice as a farmer, that the produce of my meadows has been immenſely increaſed when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the ſummer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the ſummer, in the preparation of matter calculated to afford food to the expanding buds and bloſſoms of the ſucceeding ſpring, and to enter into the compoſition of new organs of aſſimilation.

Full grown leaves perſpire moſt plentifully,

—and at this period the vegetative powers appear to be employed in increaſing the growth of the vegetable.

If the preceding hypotheſis be well founded, we may expect to find that ſome change will gradually take place in the qualities of the aqueous ſap of trees during its aſcent in the ſpring; and that any given portion of winter-felled wood will at the ſame time poſſeſs a greater degree of ſpecific gravity, and yield a larger quantity of extractive matter, than the ſame quantity of wood which has been felled in the ſpring or in the early part of the ſummer. To aſcertain theſe points I made the experiments, an account of which I have now the honour to lay before you.

If this be the caſe, it ſhould be found that the aqueous ſap muſt be altered in its aſcent; and the winter feiled wood will be denſer.

As early in the laſt ſpring as the ſap had riſen in the ſycamore and birch, I made inciſions into the trunks of thoſe trees, ſome cloſe to the ground, and others at the elevation of ſeven feet, and I readily obtained from each inciſion as much ſap as I wanted. Aſcertaining the ſpecific gravity of the ſap of each tree, obtained at the different elevations, I found that of the ſap of the ſycamore with very little variation, in different

Experiments. Birch and ſycamore in ſpring gave ſap moſt aqueous near the bottom; but denſer and more ſaccharine the higher up.

ferent trees, to be 1.004 when extracted close to the ground, and 1.008 at the height of seven feet. The sap of the birch was somewhat lighter; but the increase of its specific gravity, at greater elevation, was comparatively the same. When extracted near the ground the sap of both kinds was almost free from taste; but when obtained at a greater height, it was sensibly sweet. The shortness of the trunks of the sycamore trees, which were the subjects of my experiments, did not permit me to extract the sap at a greater elevation than seven feet, except in one instance, and in that, at twelve feet from the ground, I obtained a very sweet fluid, whose specific gravity was 1.012.

The sap first drawn was densest; which shews that its augmentation was had from matter in the alburnum.

I conceived it probable, that if the sap in the preceding cases derived any considerable portion of its increased specific gravity from matter previously existing in the alburnum, I should find some diminution of its weight, when it had continued to flow some days from the same incision, because the alburnum in the vicinity of that incision would, under such circumstances, have become in some degree exhausted: and on comparing the specific gravity of the sap which had flowed from a recent and an old incision, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding cases, the incision being made close to the ground. Wherever extracted, whether close to the ground, or at some distance from it, the sap always appeared to contain a large portion of air.

It is difficult to make experiments on the density or sp. gravity of the alburnum.

In the experiments to discover the variation in the specific gravity of the alburnum of trees at different seasons, some obstacles to the attainment of any very accurate results presented themselves. The wood of different trees of the same species, and growing in the same soil, or that taken from different parts of the same tree, possesses different degrees of solidity; and the weight of every part of the alburnum appears to increase with its age, the external layers being the lightest. The solidity of wood varies also with the greater or less rapidity of its growth. These sources of error might apparently have been avoided by cutting off, at different seasons, portions of the same trunk or branch: but the wound thus made might, in some degree, have impeded the due progress of the sap in its ascent, and the part below might have been made heavier by the stagnation of the sap, and that above lighter

Method adopted. By felling poles in an oak coppice in winter and spring and comparing them.

lighter by privation of its proper quantity of nutriment. The most eligible method therefore, which occurred to me, was to select and mark in the winter some of the poles of an oak coppice, where all are of equal age; and where many, of the same size and growing with equal vigour, spring from the same stool. One half of the poles which I marked and numbered were cut on the 31st of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to distinguish the winter-felled from the summer-felled poles, the bark being left on all, and all being placed in the same situation to dry.

The winter felled wood was densest after seasoning,

In the beginning of August I cut off nearly equal portions from a winter and summer-felled pole, which had both grown on the same stool; and both portions were then put in a situation, where, during the seven succeeding weeks, they were kept very warm by a fire. The summer-felled wood was, when put to dry, the most heavy; but it evidently contained much more water than the other, and, partly at least, from this cause, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then ascertained the specific gravity of the winter-felled wood to be 0.679, and that of the summer-felled wood to be 0.609; after each had been immersed five minutes in water.

—by more than ten per cent.

This difference of ten *per cent.* was considerably more than I had anticipated, and it was not till I had suspended and taken off from the balance each portion, at least ten times, that I ceased to believe that some error had occurred in the experiment: and indeed I was not at last satisfied till I had ascertained by means of compasses adapted to the measurement of solids, that the winter-felled pieces of wood were much less than the others which they equalled in weight.

The difference was not quite so much in the newly formed layers of each.

The pieces of wood, which had been the subjects of these experiments, were again put to dry, with other pieces of the same poles, and I yesterday ascertained the specific gravity of both with scarcely any variation in the result. But when I omitted the medulla, and parts adjacent to it, and used the layers of wood which had been more recently formed, I found the specific gravity of the winter-felled wood to be only 0.583, and that of the summer-felled to be 0.533; and trying the same experiment with similar pieces of wood, but taken from poles

poles which had grown on a different stool, the specific gravity of the winter-felled wood was 0.588, and that of the summer-felled 0.534.

It is evident that the whole of the preceding difference in the specific gravity of the winter and summer-felled wood might have arisen from a greater degree of contraction in the former kind, whilst drying; I therefore proceeded to ascertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when steeped in water. Having therefore reduced to small fragments 1000 grains of each kind, I poured on each portion six ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had sunk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infused, and had raised its specific gravity to 1.002. The specific gravity of the water in which the summer-felled wood had, in the same manner, been infused was 1.001. The wood in all the preceding cases was taken from the upper parts of the poles, about eight feet from the ground.

The winter felled wood gave out a larger portion of extract.

Having observed, in the preceding experiments, that the sap of the sycamore became specifically lighter when it had continued to flow during several days from the same incision, I concluded that the alburnum in the vicinity of such incision had been deprived of a larger portion of its concrete or inspissated sap than in other parts of the same tree: and I therefore suspected that I should find similar effects to have been produced by the young annual shoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain less extractive matter than an equal portion taken from the lower parts of the same pole, where no annual shoots or leaves had been produced.

Probability that this sap is exhausted more or less by the leaves and shoots.

No information could in this case be derived from the difference in the specific gravity of the wood; because the substance of every tree is most dense and solid in the lower parts of its trunk; and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces several equal portions of wood taken from different parts of the same poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I suffered

Experiment shewed that they leave a certain portion of extract in the trunk.

suffered to remain twenty hours, as in the preceding experiments: and I then found that in some instances the wood from the lower, and in the others that from the upper parts of the poles, had given to the water the deepest colour and greatest degree of specific gravity; but that all had afforded much extractive matter, though in every instance the quantity yielded was much less than I had, in all cases, found in similar infusions of winter-felled wood.

Hence many trees have a succession of leaves and buds.

It appears, therefore, that the reservoir of matter deposited in the alburnum is not wholly exhausted in the succeeding spring: and hence we are able to account for the several successions of leaves and buds which trees are capable of producing when those previously protruded have been destroyed by insects, or other causes; and for the extremely luxuriant shoots, which often spring from the trunks of trees, whose branches have been long in a state of decay.

The matter in the alburnum may remain inactive for several years.

I have also some reasons to believe that the matter deposited in the alburnum remains unemployed in some cases during several successive years: it does not appear probable that it can be all employed by trees which, after having been transplanted, produce very few leaves, or by those which produce neither blossoms nor fruit. In making experiments in 1802, to ascertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear tree, at a small distance from the trunk; and I pared off, at the same time, the whole of the lifeless external bark.

Instance: in an old pear tree.

The age of this tree, I have good reason to believe, somewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every successive year to terminate its existence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the succeeding spring, or early part of the summer; but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large size every where appeared, and in the autumn every part was covered with very vigorous shoots, exceeding, in the aggregate, two feet in length. The number of leaves which, in this case, sprang at once from the trunk and branches appeared to me greatly to exceed the whole of those which the tree had born in the three preceding seasons; and I

cannot believe that the matter which composed these buds and leaves could have been wholly prepared by the feeble vegetation and scanty foliage of the preceding year.

But whether the substance which is found in the alburnum of winter-felled trees, and which disappears in part in the spring and early part of the summer, be generated in one or in several preceding years, there seem to be strong grounds of probability, that this substance enters into the composition of the leaf: for we have abundant reason to believe that this organ is the principal agent of assimilation; and scarcely any thing can be more contrary to every conclusion we should draw from analogical reasoning and comparison of the vegetable with the animal economy, or in itself more improbable, than that the leaf, or any other organ, should singly prepare and assimilate immediately from the crude aqueous sap, that matter which composes itself. *It is strongly probable that this matter composes the leaf.*

It has been contended* that the buds themselves contain the nutriment necessary for the minute unfolding leaves; but trees possess a power to reproduce their buds, and the matter necessary to form these buds must evidently be derived from some other source: nor does it appear probable that the young leaves very soon enter on this office: for the experiments of Ingenhouz prove that their action on the air which surrounds them is very essentially different from that of full grown leaves. It is true that buds in many instances will vegetate, and produce trees, when a very small portion only of alburnum remains attached to them; but the first efforts of vegetation in such buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this case, no grounds to suppose that the leaves derive their first nutriment from the crude sap. *It is not likely that they are supported by the crude sap.*

It is also generally admitted, from the experiments of Bonnet and Du Hamel, which I have repeated with the same result, that in the cotyledons of the seed is deposited a quantity of nutriment for the bud, which every seed contains; and though no vessels can be traced † which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous passage, which is perfectly similar to that through which I conceive the sap to be carried from the *Seeds are thus nourished not from the soil, but from matter deposited in the cotyledons.*

* Thomson's Chemistry. † Hedwig.

leaves

leaves to the buds, in the subsequent growth of the tree; and I am in possession of many facts to prove that seedling trees, in the first stage of their existence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many instances killed, by being put to vegetate in rich mould.

(To be concluded in the Supplement.)

III.

On the Deliquescence and Efflorescence of Salts. By C. L. Cadet.*

Deliquescence and efflorescence are occasioned by the relative attractions of the air and of each salt for water.

All chemists are of the same opinion relative to the cause of the deliquescence or the efflorescence of a salt. The attraction of the salt for the water contained in the atmosphere occasions the first phenomenon, the attraction of the atmospheric air for the water of crystallization of the salt causes the second.

The difference between one salt and another has been noticed, but not the variations from the state of the air, &c.

This attraction has been found to vary in the different salts, whether deliquescent or efflorescent, to be stronger in some kinds, and more speedy in others; but no one has yet observed whether it had any dependence on the constitution of the atmosphere, the electric state of the air, the quantity of caloric it contained, if it was always the same in any one salt, and if it regularly became weaker in proportion as saturation approached, neither have any tables been yet prepared, which might indicate the degree of deliquescence, or of efflorescence of the different salts.

Of the hypotheses which could be made on these phenomena, the following seemed most probable.

Hypotheses that deliquescent salts should attract water in proportion as the hygrometer indicated its presence.

The salts which deprived the air of its humidity ought to act in this respect in proportion to the quantity of water which the air held in solution or in suspension. The greater the humidity of the air, the more should the deliquescent salts augment in weight, so that the degree of their weight should be conformable to the progress of the hygrometer.

Barometrical changes and

On the other hand atmospheric pressure, which more or less opposes evaporation, ought to have an influence on the satura-

* Journal de Physique, LX.

tion

tion of the salts, since it causes the density of the air to vary; consequently there should be an agreement between the variations of the barometer and the deliquescence of salts.

The variations of temperatures, by dilating, or by condensing the mass of the atmosphere, should also occasion changes in the proportion of water absorbed by the salts, on which account it would be useful to observe the thermometer.

—thermometrical should also have their influence.

I thought moreover that one salt had not only more or less attraction for the water contained in the air than another, but that this attraction varied likewise in the same salt in proportion as it had lost or absorbed water. I hoped by thus comparing the deliquescence and efflorescence of salts with the state of the different meteorological instruments, to obtain results sufficiently constant to establish a theory of deliquescence or efflorescence. I hoped also to be able to use the salts themselves as instruments of meteorological observation; but experience proved that reasoning apparently founded on the truest theory frequently deceives expectation. It is nevertheless necessary to attend to negative facts, which sometimes are as serviceable to science as those of a positive nature.

Deliquescent salts should attract most when least saturated.

Experience did not confirm these positions.

I did not find a single salt which seemed to have the least conformity with the state of the barometer, hygrometer, or thermometer. On the same day many salts increased considerably in weight, while others indicated a slow progress. Some had but a small attraction, when the hygrometer shewed a great degree of humidity, and were most deliquescent when the air seemed most dry. Atmospheric pressure never had the least agreement with the increase of weight of a salt, and the thermometer having varied but half a degree during the course of the experiments, does not furnish any observation on the influence of temperature. It is therefore impossible to explain by the meteorological changes any of the variations which I observed in the deliquescence, or the efflorescence of salts.

None of the salts appeared to gain or lose weight in conformity to meteoric changes.

Efflorescent Salts.

I weighed exactly 288 grains of sulphate of soda, of phosphate of soda, and of carbonate of soda, which three salts are considered as the most efflorescent, and placed them in a dry and airy situation, after having carefully dried the capsules which contained them. I put also in the same place an hygrometer, a barometer, and a thermometer: the three salts shewed the following results.

Experiments. Efflorescent salts, sulphate, phosphate and carbonate of soda exposed.

Loss of weight in each by efflorescence.

	Left to effloresce.	Lost.
Sulphate of soda -	61 days -	203 grains of water.
Phosphate of soda -	39 -	91
Carbonate of soda -	51 -	86

Considerations why the time seems to indicate no useful result in deliquescent salts exposed.

It should seem from this table that these three salts ought to be classed in the preceding order; but it must be observed that salts contain more or less water, in proportion as they crystallize slowly or rapidly. The number of days which were employed in the efflorescence of these salts should vary, both in proportion to the water they contained, and to the extent of surface which they exposed to the action of the surrounding air; and therefore the time of their efflorescence can give no appreciation of the force of their attraction for water. This reflection prevented my making experiments on any more efflorescent salts.

Deliquescent Salts.

I took 288 grains of each of the salts in the following table, (which are very sensibly deliquescent, since they all absorbed more than half their weight of water), and placed each of them in a dried capsule, along with the before-mentioned meteorological instruments, in a damp situation, and after 150 days of observations noted what is included in the table.

A Table of Deliquescent Salts, in the Order of their Attraction, estimated by the Quantity of Water absorbed.

Table of the increase of weight in each of 19 different species, and the times respectively.

	Days employed in their saturation.	Water absorbed.
Acetite of potash - - -	146	700 grains.
Muriate of lime - -	124	684
Muriate of manganese - -	105	629
Nitrate of manganese - -	89 -	527
Nitrate of zinc - -	124	495
Nitrate of lime - -	147	448
Muriate of magnesia -	139	441
Nitrate of copper - -	128	396
Muriate of antimony -	124	388
Muriate of alumine -	149	342
Nitrate of alumine	147	300
Muriate of zinc	76	294
Nitrate of soda -	137	287
Nitrate of magnesia	73	207

Acetite

	Days employed in their saturation.	Water absorbed.
Acetite of alumine - - -	104 -	202
Acid sulphate of alumine - -	121 -	202
Muriate of bismuth - - -	114 -	174
Acid phosphate of lime - -	93 -	155
Muriate of copper - - -	119 -	148

In examining this table it may be remarked that the duration of the absorption is not in any proportion to the quantity: The muriate of alumine, for example, took 149 days to absorb 342 grains of water, while the nitrate of manganese took but 89 days to absorb 527 grains. That the force of attraction may be estimated from the rapidity with which the bodies unite must not be concluded from this; for the same table shews that nitrate of magnesia saturated itself in 73 days, and only absorbed 207 grains of water, a much less quantity than that taken up by the nitrate of manganese. Although the greater or less facility with which deliquescent salts saturate themselves with water cannot be accounted for, (since a salt half saturated, or half deprived of water, is no longer the same body, and consequently exercises other attractions than what the same salt does in its ordinary state, or in a different state of saturation,) the rapidity of their saturation is not however an indifferent matter. In the experiments which have been made on producing artificial cold by muriate of lime, it has been remarked that the cold was greater in proportion as the ice was melted; but it is probable that the muriate, and above all the nitrate of manganese, which becomes liquid much quicker, would produce with ice a more intense cold, and that certain liquors which have hitherto resisted coagulation, would be solidified by these two salts, which experiment is highly deserving of a trial.

The times of absorption were not at all proportional to the quantities.

Though the rapidity of absorption does not indicate the proportional affinity yet the facts are useful.

For instance, the salts of manganese may produce intense refrigeration.

In order to examine whether deliquescence depends on the proportion of the base, or of the acid which constitutes the salts, I compared with each other the different analyses of salts published by Bergman, Klaproth, Fourcroy, and Vauquelin, and I saw that no induction could be from their composition; for there are some salts which have the base in a very considerable proportion, and which are less deliquescent than those whose base is less; and many others in which the acid is in a small proportion, are more deliquescent than those in which this principle is predominant. The nature of the acids and of the

It does not appear that the deliquescence depends on the proportion of component parts.

—nor on the peculiar nature of the ingredients themselves.

the bases themselves do not throw more light on the phenomena of deliquescence than their proportions; for there are deliquescent salts, the component parts of which taken separately, have not any remarkable attraction for water, such is the nitrate of alumine; while on the other hand the sulphate of soda is efflorescent, although concentrated sulphuric acid, and caustic soda each separately attract humidity. Nothing better proves this axiom in chemistry,—*Compounds have properties peculiar to themselves, and differing from those of their component parts.*

Generally the deliquescence was most rapid when the saturation was least.

In general deliquescent salts encrease their weight in a diminishing proportion, according as they approach saturation; thus the acetate of potash, which in the first twenty days exhibited the following progression: 21. 34. 44. 54. 60. 70. 83. 100. 110. 120. 128. 138. 142. 148. 160. 169. 177. 186. 192. 198. did not shew on the last twenty days more than this, 647. 650. 655. 660. 663. 666. 669. 671. 676. 682. 684. 686. 688. 690. 692. 694. 696. 698. 699. 670. The salts which were but little deliquescent presented a singular phenomenon, which none, I believe, has observed before.

Remarkable facts; salts which lose part of the absorbed water and afterwards attract more and encrease till saturation.

The acid sulphate of alumine, and the acid phosphate of lime, increased and diminished successively in weight.

The muriate of copper diminished during 45 days before it began to encrease. These oscillations and retrograde movements take place but once, and when the salt has absorbed a certain quantity of water, there is a progressive increase, although slowly, until its perfect satutation, which may depend on the attraction of water for water, an attraction which is not perceptible but in certain proportions.

Expediency of further experiments.

These anomalies deserve to be observed again, and compared with experiments made on other salts which do not exhibit them. They tend to make us acquainted with all the causes that produce efflorescence and deliquescence, since they present each phenomenon successively. The salts which we submitted to their action, had certainly an attraction for water very little different from that of air in a medium state of heat and humidity. The point of equilibrium must be decided by the state of the atmosphere, or the salts would remain unaltered.

These will probably shew that meteoric variations in the air

I still however think that a relation exists between the meteorological variations and the alterations of the salts; and if I was

was not able to discover it, without doubt this was caused by the small portions of salts which I exposed to the action of the atmosphere. Some chemist more fortunate will determine it, by operating on large masses, comparing experiments made in many different seasons, and keeping a register of the electrical state of the atmosphere, of the water of crystallization which the salts contain, of their division, and of the surface which they present to the air. do influence th changes in salts

In a labour which would require more than 3000 experiments, the new facts which I have observed are too few, and perhaps too little important to engage any one to undertake such prolonged and minute experiments; but I have given a table of deliquescent salts arranged according to their attraction for water, and I dare hope, that the results of it will not be altogether useless. Extensive research.

IV.

Account of the simple and easy Means by which the Harbour of Rye was restored, and made navigable for Ships of considerable Burthen. By the Rev. DANIEL PAPE *.

Memorial of Rye Harbour.

RYE Harbour, once so very safe and convenient for passing vessels up or down the channel, to run to in distress or in precarious weather, had been for many years, and from various causes, in a gradual state of decay, insomuch that in the years (I believe) 1795 and 1796, it was thought necessary to send Captain ———, from the Trinity-House, to make a survey, and report to the Board its then state, and the probability of its improvement or redemption. The survey was made, I believe, with considerable care and attention; and the result was, that the harbour was pronounced lost, or in such an irreparable decayed state that it was an useless expense to the ships passing, which paid tonnage to it; and therefore this tonnage was taken from Rye, and given to Ramsgate Harbour, leaving however a reserve in the hands of the commissioners of 6000*l.* Decayed state of Rye Harbour, in 1796. Survey and report, that the Harbour was irreparable.

* From his communication to the Society of Arts, who voted him the gold medal. See their Transactions, Vol. XXII.

The

Advertizement for plans of improvement.

The consequence of this was an advertisement, inviting any gentleman to come forward with plans for the improvement of the harbour, and the draining of the upper levels. On the day appointed for the presentation of such plans, a very sensible letter was laid before the Commissioners by the Rev. Mr. Jackson, of Rye, though impracticable on many accounts,—and also a plan by myself, proposing to make the present cut, and to form a dam of straw or hay and faggots, as represented on the chart, for the small sum of 500*l.* On reverting to the *enormous sums* that had been already, from time to time, expended by able engineers to no purpose, it was judged at the moment an impossible attempt; and, after politely voting me their thanks, the Commissioners seemed to decline carrying their plan into execution.—This, however, did not satisfy me; and therefore, confident of success, I undertook to perform what I had proposed, or lose the money, without stipulating for any fee or reward should I succeed. On entering upon this agreement, I set to work, and choosing a Mr. Southerden, an active and persevering man, as foreman, to assist me, I completed the work in three months, in the very depth of winter, at the expense of only 480*l.* though the works were twice filled up with sea-beach by the tides. But, though this was done to the astonishment and admiration of many, yet there were evidently an envious few mortified and disappointed. The cut and dam being thus finished, it was then thought necessary, on my recommendation, to secure the cut from reverting to its late reduced state, by a pier-head on the east, and jutties on the west side of it; the execution of which was committed to the eminent skill of a Mr. Sutherland, who performed the trust reposed in him, to the universal satisfaction of his employers; and I believe the whole was completed for something less than 3000*l*, in a very masterly and workman-like manner. Of this I think there cannot be a better proof adduced, than that it still stands *firm*, without the least apparent decay, and maintains its first position without the smallest variation: and no doubt a very trifling annual expense will keep it in its present improved state.

The author's plan to make a direct cut, and dam up the old mouth.

The author undertook it at his own risque,

and completed it.

Farther security by a pier head and jutties.

It proves to be perfectly durable,

The advantages derived from it are particularly great; for ships of 250 tons burden, and even vessels of 300 tons, run in with the greatest safety at spring tides: whereas, before, those of 50 tons could not come in, but with the utmost difficulty and danger.

and admit ships of five times the tonnage before admitted.

That

That part of Romney Marsh too, which lies contiguous, and was threatened by every boisterous tide with a total overflow, is now in safety, and the drainage of the levels is rendered complete.

Other advantages.

I beg leave now to offer to your attention a short description of the Dam, the form and materials of which may be used with success in similar situations, whether in places adjacent to the sea, or in gentlemen's fish-ponds, or rivers in the country, where weirs may be necessary for the preservation of the banks. The dam was merely formed of hay, straw, and faggots, pinned down to a *foundation* of *sand* or *silt* by short piles. I formed it as in the chart, of the shape of a double-roofed house, first putting down straw, and then over it hazel faggots, from 12 to 14 feet in length, and afterwards pinning down the whole with piles. I next filled the space between the two roofs with gravel or sea-beach, and secured this also with faggots pinned down upon it, over which resistance being precluded from its peculiar form, the influx and reflux of the tides glided so gently, that consequently every probability, not to say possibility, was annihilated of its being ever undermined or blown up.

Very easy and beneficial method of constructing the dam.

A double roof or covering of hay, &c. was pinned down upon a foundation of sand, &c.

The interstice was filled with gravel, and covered with faggots secured down.

It was also necessary that this dam should be put down in one tide, and that the mouth of the cut should be opened in the same time; for it was evident to me, that it was impossible ever to cut to sea in any other way. For unless the dam had been ready to turn the water through the cut as soon as opened, and the cut, on the other hand, ready to receive the current the moment the dam began to act, the whole work must have been entirely and unavoidably destroyed by the influx and reflux of the ensuing tide. All this I clearly foresaw: and by procuring a sufficient number of men, nearly three hundred, the business was completely finished, just as the tide touched the foot of the dam; and when it was full sea, the straw of course acted as a receiver and retainer to the *silt* brought in by the tide; which being repeated by each returning tide, the dam soon became entirely fixed, beyond a possibility of ever being destroyed; and it is now so entirely covered, that if the pier is kept in repair, the dam must ever remain unimpaired by time, and proof against the most violent floods of waters.

Difficulty that the old mouth should be closed and the new one opened in one single tide.

Successful result.

For this work, the Commissioners voted me fifty guineas (half of which I gave to my assistant) and alledged that, on account

The author's reward for his contrivance, and

attending the works: 50l.! account of the loss of the tonnage, and the poverty of the fund, they were sorry it was not more. This to me, under these circumstances, was a sufficient apology, and I was content. I now offer it to the consideration of the Society of Arts, as a body in some degree interested in the prosperity of this kingdom. Should they deem what I have already received an adequate compensation for such a work, and such an undertaking, at so inclement a season, I am still content. But if they should think proper to grant me an additional remuneration, it will be received with peculiar satisfaction, and considered as a very great honour by,

Sir,

Your obedient and humble Servant,

To Charles Taylor, Esq. DANIEL PAPE.

Cambridge, Trinity Hall,

April 2, 1803.

Reference to the Engraving of the Rev. Mr. PAPE's *Improvement of Rye Harbour, Plate* XIII. *Fig.* 1.

AA. The double roof, filled with straw.

BBB. Hazel faggots, 12 to 14 feet long.

C. The space betwixt the roofs filled with gravel or sea-beach.

D. The faggots which covered the gravel so laid.

E. Piles of wood driven through the faggots and straw into earth, at the bottom of the river, the heads of which piles are united by cross pieces of wood.

F. The solid bed of the river.

G. The river at low water.

H. The high-water mark.

I. The upper side of the dam, which opposes the current of the river.

K. The lower side of the dam, which resists the coming-in of the tide.

Fig. 2. L. Shows the place where the dam was placed.

M. The old course of the river represented by dotted lines, and which is now filled up with gravel by the tide.

N. The new canal, cut by Mr. Pape's directions, and which is now the regular channel for shipping.

O. The

Q. The pier head, on the east side of Mr. Pape's cut.
P P. The two jutties, on the west side of Mr. Pape's cut.
R R. The former canal, cut under the direction of Mr. Smeaton, and other able engineers; but which failed, and is since blocked up by a bank made across it, over which the present high road between Rye and Winchelsea passes.

V.

New Experiments on the Respiration of Atmospheric Air, principally with regard to the Absorption of Azote, and on the Respiration of the Gaseous Oxide of Azote. By *Professor* Pfaff, *of Kiel* *.

Short history of chemical researches concerning respiration.

THe great discoveries in pneumatic chemistry, the ingenious and useful applications of these discoveries to explain the phenomena of organized beings, particularly the animal economy, and the valuable researches of eminent philosophers have greatly contributed to throw light on the doctrine of the chemical effects of respiration. In consequence of these researches, phisiologists are in general agreed with regard to the most essential points of this doctrine; such as the production of carbonic acid, the use of oxigen gas, and the animal heat which results from its absorption. But the activity of philosophical enquirers has not yet succeeded in removing all the obscurities of this subject, and the disagreement between the results of various experiments relating to them, sufficiently shew, that new enquiries are requisite to ascertain the sources of such errors as still continue, and to remove them altogether. The experiments of the celebrated Davy have done much in this respect, and the researches on the nitrous oxide afford a new epocha in the chemical doctrine of respiration. The celebrated editors of the Bibliotheque Brittannique, have shewn their conviction of the great value of these researches, by the ample and instructive extract they have given, and the manner in which Berthollet in the 45th volume of the *Annales de Chemie* has given an account of the same, has sufficiently fixed the attention of philosophers on that excellent work.

Davy on nitrous oxide.

* This memoir was addressed to the French National Institute, and read at their sitting of the 25th of Messidor last (July 13.)

The

The differences which existed in the results of former experiments, as to the quantity of carbonic acid produced in the act of respiration were less important, and might entirely depend on the constitution of the different individuals upon whom the experiments were made; and under this point of view, a revision of the experiments was less necessary. But the part which is performed by azote gas in the act of respiration has been too little attended to. It has been generally supposed to be altogether without activity. Goodwin alone thought he had observed a considerable absorption of azote gas; but his experiments were not made with all the necessary accuracy, and were too directly opposite to the experiments of Lavoisier, Seguin, Abernethy, Fothergill, Menzies, &c. to fix the attention. The experiments on the slow combustion of phosphorus, which does not succeed in pure oxigen gas, but is so greatly forwarded by the presence of the azote gas of atmospheric air, shew to a certain degree the advantages which this great quantity of azote gas is likely to produce in respiration; and the unfortunately too concise results of the last experiments of the immortal Lavoisier on respiration, in which it was found, that a much greater mass of oxigen gas is decomposed in the same time by respiration in atmospheric air than in oxigen gas, stand in confirmation of the former fact. But hitherto we have possessed only probabilities, or results not sufficiently connected with the subject. To Davy it is that we are indebted for an exact and incontestible knowledge of the active part which azote gas performs in the process of respiration. But in proportion to the novelty and interesting nature of these results do they require to be confirmed by the experiments of others; and it was in this point of view that I undertook last winter a series of experiments upon respiration in atmospheric air, and also in the gaseous oxide of azote; the principal results of which I now venture to communicate to the National Institute.

Azote necessary for the slow combustion of phosphorus.

Experiments on the Respiration of Atmospheric Air, and Oxigen Gas.

Experiments of respiration.

All the following experiments were made in the academical laboratory of the University of Kiel, which is provided with all the accurate apparatus of modern chemistry. They were made

made for the most part in the presence of my pupils, particularly one named *Dierks*, who was most commonly the subject of experiment.

In order to determine with precision the changes which atmospheric air undergoes by respiration, and to decide respecting the absorption of azote gas, we must begin with ascertaining the diminution which a given volume of air undergoes by respiration. This first point was to be determined by accurate experiments.

Quantity of diminution undergone by air by the process of respiration.

1. The quantity of 170 duodecimal cubic inches of Paris* were respired from one of the great reservoirs of a gasometer, constructed at Paris after the model of that of Charles, over water covered with oil, to prevent the absorption of the carbonic acid gas produced by respiration. The respiration was performed once only during the time of ten or twelve seconds. The diminution was 4,72 cubic inches, or $\frac{1}{36}$ part of the first volume. This experiment being repeated twenty times in the same manner, afforded the same result.

2. 144 Cubic inches were respired once in the space of ten or twelve seconds. The diminution was four cubic inches or $\frac{1}{36}$ part of the primitive volume.

3. The same volume was respired twice during 22 seconds, and the diminution amounted to eight cubic inches, or $\frac{1}{18}$ part of the primitive volume. The same volume having been respired three times during 30 seconds, the diminution amounted to 12 cubic inches, or $\frac{1}{12}$ part of the primitive volume.

4. 60 Cubic inches were respired three times during 25 seconds, the diminution was six cubic inches, or $\frac{1}{10}$ of the primitive volume.

5. 170 Cubic inches were respired four times during one minute, and the diminution amounted to 20 cubic inches. This experiment was several times repeated; and the diminution was almost constantly the same. Namely, 18, 19, 21, and 20 cubic inches, or $\frac{1}{8,5}$.

6. 168 Cubic inches respired during 50 seconds, by four great and four small respirations, suffered a diminution of 14, or $\frac{1}{12}$ of the primitive volume.

7. 430 Cubic inches by 12 respirations in 90 seconds, suffered a diminution of 24, or $\frac{1}{18}$ part.

* As these quantities are merely relative, I have not reduced them. T.

These

These results agree very well with those obtained by Davy on the diminution of air by respiration. He found the diminution by one single respiration to be $\frac{1}{7_3}$ part, and by respiration continued for one minute $\frac{1}{3_1}$ part.

The magnitude of the diminution depends not only on the time during which a given volume of air is respired, but principally on the magnitude of this volume itself; it must be proportionally less the greater the quantity of air inspired. A very essential error is seen in the results of Abernethy, who gives a greater volume to the expired than to the inspired air; and the calculations of Goodwin are founded on a mistaken basis; for he supposes the two volumes equal.

Diminution of oxigen gas by respiration.

In order to determine comparatively the diminution of oxigen gas by respiration, 170 cubic inches of oxigen gas obtained from manganese were respired in the same manner, and under the same circumstances as the 170 cubic inches of atmospheric air in the 5th paragraph. The diminution amounted to 30 cubic inches, and in other experiments, to 33, 29, 31. The mean term of which is $\frac{2}{11}$ parts of the primitive volume.

This diminution being established with accuracy, may be applied to determine the absorption of azote gas.

Experiments to determine how much azote is absorbed in the process of respiration.

8. 80 Cubic inches were respired one time slowly during ten or 12 seconds, and the air expired was received over mercury.

The relative quantity of the constituent parts of this respired air was in the centenary 4,16 carbonic acid, 16,55 oxigen gas observed by the slow combustion of phosphorus, 79,19 of azote gas. An eudiometric experiment made at the same time, gave the following proportion of the parts in atmospheric air, one carbonic acid, 21 oxigen gas, and 78 azote. The total diminution of the air was from the preceding experiments $\frac{1}{3_6}$. We may therefore find the true quantity of azote gas by the following proportion, 36 : 35 :: 79,19 : 76,99. If we subtract this 76,99 from 78, the primitive quantity of azote in the atmospheric air before respiration, we find a loss of 1,01 on the hundred parts of the whole mass of air breathed. But as the quantity of air inspired was really no more than 80 cubic inches, the absolute diminution or disappearance of azote gas by one respiration, must

must be diminished in the same proportion of 100 to 80, and thus proves 0,808 cubic inches.

9. In another experiment 60 cubic inches were respired once in the time of 10 or 12 seconds, and the last portion of the expired air was received over mercury. The proportion of the constituent parts after respiration, were in the centenary 4,68 carbonic acid gas, 17,68 oxigen gas, and 77,74 azote gas. An eudiometric experiment made at the same time on the atmospheric air, gave 1 carbonic acid, 22 oxigen gas, and 77 azote gas. The true quantity of azote gas found as before, by diminishing the 77,74 $\frac{1}{36}$, is, 75,58. And this being subtracted from 77, the quantity of azote gas previous to the respiration leaves 1,42 for the azote which disappeared, supposing the respired air to be divided into 100 parts. But if we take the real number in inches, which was 60, this quantity will be expressed by 0,852 cubic inches.

10. 30 Cubic inches were respired in the same manner three times during 16 seconds. The expired air contained in the centenary 5 carbonic acid gas, 14,5 oxigen gas, and 80,5 azote gas. The atmospheric air contained by experiment at the same time, 1 carbonic acid gas, 20,75 oxigen gas, and 80,025 azote gas. This by the same process of computation gives a diminution of 4,235 in the 100, or in cubic inches 1,2705.

Remarks on uses, &c. of azote.

These experiments which were several times repeated, and constantly with the same result, decisively shew that azote gas is absorbed in the act of respiration, and the active part it performs. Hence we may more easily understand, why azote gas compared with other mephitic gases is so little noxious to our lungs; so that according to the experiments of Lavoisier and Seguin, animals live very well in a mixture of 15 parts azote gas, and one part oxigen gas; whereas the same animals were suddenly suffocated in a mixture of 40 parts oxigen gas, 45 azote gas, and 15 carbonic acid gas. Hence we may comprehend, at least to a certain extent, the extraordinary effects of the gaseous oxide of azote; we may form some notion of the transformation of the chyle, which is less anamalized or azotised in the lymphatic part of the blood, but becomes more so in the act of respiration. But the quantity of azote gas absorbed by one single respiration is not very considerable,

which

which agrees perfectly with the experiments of Davy, who found that no more than 5,1 cubic inches of azote gas were absorbed by 19 respirations of a volume of 151 cubic inches.

Experiments shewing the quantities of carbonic acid produced in respiration.

11. To determine the quantity of carbonic acid gas produced by the respiration of atmospheric air, 60 cubic inches were respired once during ten or twelve seconds, and received over mercury when expired. Lime water absorbed 4,08 parts in 100. This experiment being several times repeated gave the same result. The last portion of expired air being several times transferred through lime water was diminished 4,9 parts in 100.

12. 20 Cubic inches respired three successive times during 10 seconds afforded no more than five hundredths of carbonic acid gas.

13. 170 Cubic inches were respired four times during 50 seconds, the quantity of carbonic acid gas obtained was 5,8 hundredths.

14. 170 Cubic inches were respired from a bladder eight times in one minute. Lime water absorbed 8,2 hundredths.

This quantity of carbonic acid produced by respiration, afforded a term of comparison to ascertain the quantity of the decomposition of oxigen gas in respiration from the same quantities of atmospheric air, and of pure oxigen gas.

Oxigen gas produces more carbonic acid in respiration than atmospheric air does.

The preceding experiments (7) had shewn that the diminution of oxigen gas was more considerable than that of atmospheric air. From this fact it might be expected, that the production of carbonic acid gas would likewise be more considerable; and this was confirmed by direct experiment.

15. 170 Cubic inches of oxigen gas obtained from manganese, were respired four times during 50 seconds; the diminution was 30 cubic inches. The quantity of carbonic acid produced was 8,2 hundredths. Atmospheric air respired in the same manner, and under the same circumstances, gave only 5,8 carbonic acid.

16. 70 Cubic inches respired from a bladder during 50 seconds, also gave eight hundredths of carbonic acid.

Experiments on the Respiration of the Gaseous Oxide of Carbon.

Observations on the method of obtaining gaseous oxide of azote.

The gaseous oxide of azote was obtained by the process of Davy from crystallized nitrate of ammonia. This nitrate of ammonia affords very different products in different temperatures.

peratures. I have made a considerable series of experiments on this subject, which I shall shortly submit to the National Institute. I shall only remark in this place, that the oxygenated muriatic acid is obtained at the commencement, if the nitrate of ammonia be not entirely free from muriatic acid; that at a temperature not exceeding 220 degrees of the centigrade thermometer, the gaseous oxide of azote is obtained in great quantity, and very pure, without any mixture of those white vapours which have the taste of mustard; but that a temperature still higher, especially at a red heat, the gaseous oxide of azote is no longer disengaged but nitrous gas is formed, and very peculiar white vapours which I am at present examining. To prevent any explosion, I always mix the nitrate of ammonia with very pure sand. To obtain the gaseous oxide of azote in a very pure state, the distillation must be made on a sand bath, and the fire carefully managed. When every thing succeeds properly, the gas is so pure, that it may be respired immediately; it has an agreeable taste, almost sacchar vinous. If it be mixed with the white vapours produced by too strong a heat, time must be allowed for them to be deposited. The effects which Davy has observed, and Pictet has described with so much interest in his second letter in the 17th Volume of the *Bibliotheque Britannique*, were perfectly confirmed in my experiments. Several persons who respired this gas were exalted absolutely in the same manner. One of those who respired it was very speedily intoxicated, and put into a very extraordinary and most agreeable extacy. Others resisted somewhat longer; one only seemed to be scarcely at all affected. The exaltation always passed over without leaving any perceptible relaxation. I still continue these experiments. Perhaps this gas may become a powerful remedy for melancholy affections. I shall not fail to communicate the results of my experiments to the National Institute.

Davy's experiments succeeded perfectly with our author.

VI. *Experiments*

*Experiments on Gum Arabic and Gum Adracanth. By M. Vauquelin**.

Red gum adracanth left after combustion 3½ hundredths chiefly carbonate of lime, with a little iron and phosphate of lime.

TEN grains of red gum adracanth produced on combustion three decigrams and a half of white ashes. These ashes dissolved in muriatic acid with effervescence, and gave forth an odour of sulphurated hidrogen. Their solution deposited a precipitate by ammonia, which was phosphate of lime and oxide of iron. The oxalate of ammonia precipitated from it much lime. Thus red gum adracanth contains in 100 parts about 3½ of ashes, which was composed for the most part of carbonate of lime, a small quantity of iron, of phosphate of lime, and perhaps of a very minute portion of alkali.

White gum adracanth left a residue of 3, which contained the same principles and alkali.

2. Ten grains of white gum adracanth submitted to the same proofs, gave three decigrams of ashes, which were composed of the same principles as the red kind, with the addition of a little potash.

Gum arabic left 3 containing no alkali.

3. Ten grains of gum arabic burnt as the others, left three decigrams of ashes, which were composed of the same elements as the preceding, except that they gave no sign of the presence of alkali or of sulphur.

Opacity and difficult solubility of gum adracanth.

I formerly thought that the opacity of gum adracanth, and the difficulty of its solution in water, might be occasioned by a greater proportion of earthy matter; but after these experiments it appears, that they are due to another cause.

The lime in gums is combined with acid, forming a soluble salt.

The lime which I found in the gums which I am about to mention, was doubtless neither in the state of carbonate, and still less in that of quicklime; for the solutions of the gum were not in the least alkaline, but on the contrary, slightly acid; at least a bit of the gum rubbed on some paper well moistened (with blue vegetable juice) made it slightly red. It is also certain, that oxalate of ammonia and carbonate of potash occasion precipitates in the solution of gum arabic, and that acetite of lead does not form any. It follows from this, that the lime is most probably united to some acid in the gums, which doubtless is a vegetable acid; for in being decomposed they leave their bases combined with carbonic

* Annales de Chimie, Tom. 54.

acid;

acid; but can be neither the oxalic, the tartarous, or the citric, because their combinations are insoluble in water, and that besides they exist but in a small number of vegetables; still less can it be the benzoic, the gallic, the *moroxalic*, or the *laccic*, which are very rarely found in naturally, and of which the three last also form insoluble compounds.

The acid must be either the acetous or the malic.

There only remains to decide between the acetous and the malic acids, which are the most abundant in the vegetable kingdom. The first forms, as is well known, soluble combinations with all the substances with which they are capable of union; some of them are even deliquescent. It is besides the most frequent result of the operations of natuse in the vegetable and animal systems, since it is formed by vegetation, by fermentation, the action of the more powerful acids, and by the influence of heat.

But the malic forms insoluble compounds with lime,

The combinations of the second are for the most part insoluble in water; that which it forms with lime particularly, is not sensibly soluble, but when there is an excess of acid; and its existence in nature is by no means so frequent as that of the acetous acid; and as the lime which is found in the transparent gums has been incontestibly dissolved in the juices of the vegetables which produce these substances, it is much more probable, that this earth is in them combined with acetous acid than with any other.

consequently the acid is more likely acetous.

The potash in gums also forms an acetite.

It is also probable, that the small quantity of potash which I found in the ashes of the burnt gums, is united to the same acid, which explains why these substances are so sensible to humidity, and soften so much as to prevent their pulverization.

Some gums contain lime in greater proportions.

I am, however, much inclined to think, that in certain opaque adracanth gums, which are of difficult solution, and yield much lime on incineration, this earth is combined with malic acid. I have had occasion lately to examine a gum gathered by M. Palissot Bauvois, from the cochineal nopal, which was opaque, swelled with water, did not dissolve uniformly, and which yielded eight per cent. of lime. And as the sap of every cactus which I have analyzed, yielded more or less acidulous malate of lime, there is great reason to believe, that the species of it which nourishes the cochineal contains it likewise: and that it is the presence of this salt proceeding from the vegetable, and dissolved in the sap along

General results. with the gum, which causes its opacity, and obstructs its solution in water. It results at least from these experiments, that the gums contain, first a calcareous salt, most commonly the acetate of lime; secondly, sometimes a malate of lime with an excess of acid; thirdly, phosphate of lime; fourthly and lastly, some iron which is probably also united to phosphoric acid.

VII.

Method of obtaining Cobalt pure. *By* M. TROMSDORF*.

Zaffre detonated with charcoal and nitre.

FOUR parts of zaffre well pulverized are to be mixed carefully with one part of nitre, and half a part of charcoal in powder: this mixture is to be projected in small quantities into a red hot crucible, and this operation repeated three times, adding each time to the residue new portions of the nitre and the charcoal.

Fusion with black flux.

The mass resulting from these detonations ought to be mixed with one part of black flux, and exposed during an hour in a crucible to a red heat.

The metallic button again detonated.

The whole is then to be left to cool; the metallic cobalt to be separated, pulverized, mixed with three times its weight of nitre, and the mixture detonated with the precautions above mentioned.

Lixiviation separates the acid and arsenic.

The iron contained in the cobalt will thus become strongly oxidated, and the arsenic acidified combines with the potash: The mass pulverized is to be lixiviated many times, and repeatedly filtered; and thus the arseniate of potash formed will be separated from the insoluble residue that contains the cobalt,

Nitric acid dissolves the cobalt alone.

This residue is then to be treated with nitric acid, which dissolves the cobalt without attacking the iron which is found oxidated to its *maximum* of oxidation,

Evaporate and redissolve.

The solution is then to be evaporated to dryness, the residue redissolved in nitrous acid, and the liquor filtered, to separate the last portions of the oxide of iron, which might have escaped in the first operation.

* From the Annals de Chimie, Tom. 65.

All

All that remains to be done after this is, to decompose the nitrate of cobalt by potash, to wash the precipitate, and to effect its reduction by means of heat. Precipitate the cobalt by potash and reduce.

VIII.

A new Method of extracting Sugar from Beet-Root. *By M.* ACHARD *.

THE roots of the beet, after being properly cleansed, are sliced and pressed. The juice obtained is thick, and of a deep colour: it contains, besides sugar, albumen, fecula, and other matters from which it must be cleared, in order to obtain the pure sugar. In this process of separation it is, that the art of procuring sugar from the beet-root consists. Beet roots are sliced, pressed, and the juice separated.

In a boiler of tin, or of tinned copper, mix 100 lbs. of the juice of beet root with 3¼ ounces of sulphuric acid diluted with one pound of water; then pour it off, and let it stand for 12, 18, or 24 hours; 12 hours are sufficient, but 24 will not be detrimental to the process, as the acid prevents any change in the juice. In order to separate the sulphuric acid, put into the liquor 7½ ounces of wood ashes, to which add soon afterwards, 2 ounces or 6½ drachms of lime slaked in water. The sulphuric acid coagulates the albumen, the wood ashes, consisting chiefly of lime, and the lime itself separate in their turn the acid, in form of an almost insoluble salt. It will here be recollected, that in the West Indies, in the fabrication of coarse sugar, and in the refining houses of Europe, lime is used to assist the separation, and the crystallization of this article. One 56th part of sulphuric acid is added to the juice, and after standing 1-12th of wood ashes, and one part of lime. The albumen, &c. are thus separated.

After this first operation the beet root must be clarified; for which purpose it must be poured into a boiler so placed as that the fire may act equally upon all the whole surface of the bottom, in which it is to be heated to a state bordering upon ebullition, but must not be suffered actually to boil. After drawing out the fire, the syrup is to be skimmed till the skum arises in blackish flakes. The liquor is now to be filtered through flannel, which must be done with caution, lest the dregs pass through with the syrup. The skum and the dregs are good for fattening swine. Farther purification by boiling, scumming, and straining.

* Van Mons's Journal, Vol. VI.

 The

Brisk evaporation.

The syrup, thus clarified and filtered, is placed in a shallow cauldron, to the depth of not more than six inches, and evaporated over a brisk fire, whereby it is prevented from becoming a liquid saccharine mucus, which resists all attempts to crystallize it.

Cooling and farther purification by subsidence.

When reduced to about one half of its quantity, the syrup is to be poured into tin vessels about six feet in height, and half a foot in diameter, with cocks about six inches from the bottom. It must here be left for two or three days, during which time it precipitates whatever remaining impurities it may contain, particularly gypsum. At the end of this period, the liquid may be drawn clear off, and replaced in the shallow boiler, but only to the height of three inches, to evaporate; the fire to be gradually augmented, as the syrup thickens, until it be in a state of ebullition. The fire is then to be damped to prevent the sugar from burning, which would render it unfit for crystallization.

Evaporation till the syrup draws a thread.

When the syrup becomes fibrous, the fire is to be extinguished.

Crystallization or graining in a warm apartment.

In about half an hour afterwards, the syrup is to be poured into cones, of which the mouths are stopped with linen cloths, and containing a little coarse sugar-candy, grossly pounded. These cones are set in a room whose temperature is from 10° to 20° of Reaumur.

When the several operations have been dexterously managed, the sugar will crystallize in 24 hours: but if the evaporation, or baking, has been too hasty, the whole becomes a granulous mass, with the interstices filled with melasses.

The melasses drawn off

When the sugar is well crystallized, the mouth of the cone is to be opened, and an earthern vessel placed under to catch the melasses: this operation, according as the syrup has been more or less baked takes three or four weeks.

leave coarse sugar.

The substance remaining in the cones, of a yellow colour, more or less tinged with white as the baking has been well or ill conducted, in granulated crystals of various sizes, is the coarse sugar of beet-root.

Improvement.

Mr. Achard, in order to save time, and to avoid the use of vessels for settling the liquor, afterwards deviated from his original plan, by adding to the syrup, when half evaporated as above described, for every twelve quintals of roots used, five

five quarts of skimmed milk, and shortly afterwards one quart of vinegar. He then proceeded with the second evaporation in the boiler.

This sugar by refining may be made to answer all the uses of that of the West Indies, and may be rendered equally white by the usual process. Subsequent refining as usual.

IX.

On Nickeline (Niccolanum), *a Metal in many Respects resembling Nickel, lately discovered by Dr.* J. B. RICHTER.*

I HAVE long since conjectured in analysing the cobalt ores of Saxony, that they contained, besides cobalt, arsenic, copper, nickel, and iron, another metal which resembled nickel in many of its properties, but the means which I have hitherto employed to separate it did not before afford me any satisfactory results. Suspicion of a new metal in cobalt.

I was chiefly surprised that nickel, after being purified by the liquid process from cobalt, iron, and arsenic, and after that reduced without the addition of a combustible body, never formed a mass, but was always found dispersed in small particles in a hard heavy mass, which had the appearance of the remains from vitrified copper. Remarkable fact, that nickel cleared of iron and arsenic and fused, is almost dispersed in small globules

This hard matter had no metallic lustre, neither was it attracted by the magnet: Its colour was of a blackish grey on the surface, with a small degree of brightness; and in powder it was brown, greyish, and greenish. through a hard, heavy, blackish-grey mass.

Some weeks ago I endeavoured to reduce *per se* almost half a pound of oxide of nickel, which I had purified as well as possible by the liquid process, for the greatest part of a year, at a considerable expence: as this oxide was not of a lively green, I thought this was caused by the "extractive matter" which might be in the potash employed for the precipitation of the sulphate of nickel from the ammoniacal preparation: it is true that this triple combination had not that beautiful grass-green colour which it commonly had; but I thought this Experiment with a large quantity of the oxide of nickel.

* *Annales de Chimie*, LXIV.

might

might be caused by the substitution of the potash to the ammonia mixed with the copper, which could not be separated but by the reduction *per se*.

Only a small quantity of nickel in a mass was obtained.

From these ideas I hoped to have at least four ounces of perfectly pure nickel, but was disagreeably surprised by finding in the crucibles, which were deformed in the usual manner, and perforated by the vitrified copper, a rough mass with the appearance I have before mentioned, and which contained only a morsel of about two and a half drams, and consequently only five drams of pure nickel in the two crucibles. I reduced to powder in an iron mortar the remaining mass (which could not properly be called scoriæ), and separated from it by the sieve and the magnet, the particles of nickel which it might contain, which produced near two and a half drams more; and that nothing might be lost, I treated the powder with nitric acid, which attacked it vigorously at the first, and gave a solution of nickel, but after that did not act on it in the least, so that the powder was but little diminished in weight: in exposing this matter to reduction *per se*, it produced no regulus, but merely agglutinated its parts.

The dense accompanying matter was then pulverised and the nickel separated by the magnet and by nitric acid. Strong heat applied to the mass gave no more regulus.

The mass being again powdered was urged with charcoal, and afforded more than half its weight of metal in one mass.

Having again pulverized the mass, which weighed almost $4\frac{1}{2}$ ounces, I mixed with it one ounce of charcoal in powder, and exposed to the fire of a porcelain furnace during eighteen hours, in a crucible closed with a luted cover, in a part of the furnace which seemed to me to have most heat. After having broken the crucible, which was in a sound state, I found, under a scoria of a deep blackish-brown colour, a well fused button of metal which weighed two ounces and three quarters: it was not at all connected with the adjoining parts of the scoria, and had at its inferior part a particular shape, which was caused by cavities which were not produced by the crucible.

It was steel colour, rather hard, scarcely malleable, magnetical, &c.

This metal had the grey colour of steel, inclining a little to red: it presented in its fracture a grain not very fine: it was rather hard: could be extended a little under the hammer in a cold state: heated to redness it endured little the strokes of the hammer: it was attracted by the magnet, but not so strongly as either iron or nickel: it had many properties common to nickel, but it was distinguished from it entirely by others. As many of these properties were such, that those

not

not well acquainted with nickel in its perfectly pure state might take it for that metal, I have called it Nickeline (*Niccolanum.*) Name Nickeline.

The nickeline was free from all the metals which are found in the cobalt ores, except a little copper.

The specific gravity of cast nickeline, which enters more readily into fusion than nickel, is 8,55; and of forged nickeline 8,60. On putting it into nitric acid and heating it, it is attacked more quickly than nickel: I remember having observed an equally violent action of nitric acid on nickel reduced by charcoal, which I then considered as pure, and which I dissolved in order to precipitate from it by potash an oxide, which I might reduce *per se*. Specific gravity 8,6. Nitric acid dissolves it.

The solution of the nickeline went on well; being come to the point of saturation, it had a blackish-green colour, and assumed a gelatinous consistence.

I employed my first care to separate from it a part of the iron which I thought it contained, and left it to dry a little over a spirit lamp: the mass became continually of a deeper green, and in approaching to dryness it gave out much red vapours, and the residue became of a blackish grey; I added distilled water to it, which dissolved but little of it, and that which was dissolved was an insignificant quantity of nickel. Separation by drying of the acid. Residue a black powder.

I poured muriatic acid on the blackish powder well washed, which gave a green solution, in disengaging a strong odour of oxigenated muriatic acid. Soluble in muriatic acid. Green solution; which when dried gave a reddish mass that turns green by damp.

The muriatic solution was, as well as the nitric solution, of a deep blackish grass-green colour: being evaporated to dryness, it produced a reddish mass, which became green in a moist air, and which communicated the green colour to water in which it was dissolved.

This dark-coloured oxide of nickeline was insoluble in nitric acid, and in sulphuric acid; but if sugar or alcohol was added, the solution took place with facility at the boiling point. Dark oxide of nickeline not soluble in nitric or sulphuric acids without combustible matter.

The sulphate of nickeline, being combined with water, is also of a blackish green; but it assumes a pale red colour on being deprived of the water. Sulphate of nickeline.

If carbonate of potash be added to the preceding solutions of nickeline, it occasions a precipitate of blue carbonate of nickeline, inclining a little to grey and green, and of a pale tint: This combination is very light and soft, and dissolves in the Precipitate by carbonate of potash;

the acids with a strong effervescence. I remember to have had, some years ago, this precipitate of a bad colour, and not then to have examined it, considering it as a mixture of iron, nickel, and arsenic, (which last continually made itself noticed by its odour of garlic): But at last I suspected its nature.

by caustic potash;

If the solution of nickeline is decomposed by caustic potash, it gives a precipitate which resembles in its colour carbonate of chrome; that is to say, it is of a deep greenish-blue, which does not change when it is washed: being dried with a gentle heat, it assumes a pale colour, which becomes deeper when it is moistened with water.

by ammonia.

If any of the foregoing solutions of nickeline is mixed with ammonia to excess, the liquor assumes a pomegranate red colour, and remains transparent; which proves that it does not contain any iron, beause that this latter is not soluble in ammonia. By candle-light this solution is with difficulty distinguished from that of perfectly pure nickel; but by daylight, this latter is of an amethyst red colour, as I have elsewhere remarked.

Points of comparison between nickeline and nickel or cobalt.

I shall now compare the principal properties in which nickeline resembles altogether, or in part, nickel or cobalt, and those in which it is distinct from them.

It resembles cobalt—

Resemblances of nickeline and cobalt.

1. By its property of super-saturating itself with oxigen at the expence of the nitric acid, and thus forming a body which resembles the black oxide of manganese with regard to its solubility in the acids: 2. By its property of not being reducible but by the intervention of a combustible body.

It differs from cobalt—

Differences between nickeline and cobalt.

1. By the blackish-green colour of its solutions, even when they are entirely neutralized. It is known that the neutral solutions of cobalt in the sulphuric, nitric, and muriatic acids, are of a crimson-red colour; and that the muriate of cobalt alone becomes of a greenish-blue on being deprived of its water: from whence it happens that an excess of acid produces this colour, because it combines with the water: With the muriate of nickeline precisely the reverse takes place; when mixed with water it is green (although of a less beautiful colour than the cobalt without water), and when deprived of its water it becomes reddish.—2. By the colour of its

its carbonate: that of cobalt is of a beautiful poppy-blue, but the carbonate of nickeline is a bluish-green inclining to a pale grey.—3. By the colour of its oxide precipitated without carbonic acid: that of cobalt is of a deep blue, and changes on washing to a blackish-brown; but this oxide of nickeline is of a greenish-blue, and its colour does not change.

Nickeline resembles nickel—

1. By its strong magnetic quality; although this is not so great as that of nickel.—2. By its malleability, which however is less than that of nickel.—3. By the deep green of its solutions; although this colour is not so beautiful as that of the solutions of nickel.—4. By the loss of this green colour when its neutral combinations are deprived of water.—5. By the colour of the acid solution with an excess of ammonia, which cannot be well perceived by candle-light. Resemblances between nickeline and nickel.

Nickeline differs very distinctly from nickel—

1. Because it cannot be reduced without a combustible body added to it.—2. Because nitric acid attacks and oxidates it more easily. Nickel is not near so readily acted on by the nitric acid if it is not mixed with the nickeline, which almost always happens with the magnetic nickel which is considered to be in a state of purity, and which has not been reduced *per se* before my discovery.—3. It also differs from nickel by the property first mentioned of those in which it resembles cobalt.—4. By the colour of its combinations with the acids, when deprived of water: This colour in nickel is almost a buff *(chamois)*, and in nickeline a reddish, except in the nitrate of nickeline, which cannot be deprived of water without decomposing it.—5. By the colour of the precipitates, mentioned in the second and third articles concerning the properties wherein this new metal differs from cobalt, which are in those of nickel of a green colour entirely different from those of nickeline, which latter are of a much more agreeable green, especially those of the carbonate. Differences between nickeline and nickel.

Letter

X.

Letter from G. CUMBERLAND, *Esq. on a Project for extended Roads on the Principle of the inclined Plane.*

To Mr. NICHOLSON.

SIR, *Oct.* 26, 1805. *Weston-supra-Mare.*

Account of rail-roads, ABOUT ten years ago having frequent occasion to remark, and suffer from, the miserable state of the roads from Staines to London during the winter season, I ventured to propose (not having at that time either seen or heard of rail-roads) a plan which I called a *truck-road* for the whole of that stage, because it was intended to convey all sorts of goods and even carriages on trucks, going to town on one side of the old road and returning by the other.

communicated to Dr. Anderson. This plan I sent some time after to Dr. Anderson, with a drawing, for his *Recreations*, but by some accident it was mislaid and lost; and the reward of my trouble was the sly sneers of my grave Windsor neighbours, to whom it was known, accompanied with a sort of pity for heads capable of proposing such eccentric inventions.

Time however revenged my cause, by showing them the practicability of such schemes in the progress of the Surry undertaking.

Another plan by an inclined road. At the same time *another* plan of expeditious conveyance occurred to my mind, but which I was deterred from then producing owing to the cold reception my first contrivance met with—And as no one, as far as I can learn, has hitherto brought forward any improvement of the kind (although so very obvious that it might easily be suggested to the mind of a child who had heard of roads on inclined planes,) I take the liberty to recommend it to your patriotic publication, convinced that whatever may, at one time or other, be of service to mankind, will be always sure of a favourable reception at your hands.

Particular detail. Dispatches may be rolled in a spherical case down a long inclined channel. The plan I propose then is this:—That all dispatches and post-letters may, wherever it is compatible with the inclination of the ground, be conveyed ten or fifteen miles to and from London by means of *iron or wooden shells of a globular form, rolling*

rolling in a cylinder of brick or ſtone. When cloſed and locked, a due momentum being given at a proper elevation, it is eaſy to ſee that their ſpeed and ſecurity muſt far ſurpaſs any other mode of conveyance that we at preſent know of; and all that would be neceſſary in addition to the machine would be to have proper beds of ſand or wool bags to blunt their projectile force at the end of their career.

I ſhall not at preſent enter into the diſcuſſion of the conſtruction of the tube-road, or go to a calculation of their expence; but if you think the bare hint worth publiſhing it will give me pleaſure, ſhould the idea be approved, to go into all the minutiæ of their utility in other reſpects, and the means of their ultimate accompliſhment;

Being always, Sir,

Your obliged humble ſervant,

G. C.

XI.

On the State of Provincial Societies for Scientific and Literary Improvement. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Great advantage that would reſult to the town of Aberdeen from the eſtabliſhment of a public library, &c.

BEING in the cuſtom of viſiting Aberdeen, in one of my laſt tours, I inquired if there were any Antiquarian or Literary Society, or Subſcription Library there, and was much ſurprized to find neither one nor the other; there is, I was told, an Athæneum, in which a good number of newſpapers, and ſome of the moſt reſpectable periodical publications, are taken in, and in a room above that, a circulating library; this laſt I knew to be the property of two very reſpectable bookſellers in Aberdeen, and I believe the former is alſo, but the two united by no means effect the utility of either a literary ſociety, or a *ſubſcription* library, *in which the books, &c. are the property of the members, and whoſe concerns, ſuch as chooſing and ordering books, and the like, are conducted by a committee, choſen out of the ſubſcribers.* Few of thoſe who know that there is no ſuch inſtitution there, when they conſider the reſpectability of the place, either in a commercial or literary view,

view, but must feel greatly astonished; and more particularly will the want appear, when it is also known, that in Montrose, Arbroath, Dundee, and Perth, places much smaller than Aberdeen, and not possessing any college establishment, there are *subscription* libraries, *on the above plan*; nay, that Perth hath also an Antiquarian Society! Subjoined is a list of some other places in North Britain, enjoying the advantages of such establishments as I would recommend to Aberdeen; some of whom, it is obvious, have not near the prospect of success that that place could command.

Glasgow, Paisley, Greenock,
Kilmarnock, Linlithgow, Haddington.
On the borders of Northumberland, Dunse and Kelso.

The annual subscription to none *of these is more*, in some cases *not so much*, as to the Athæneum of Aberdeen and others, and they all possess very excellent and increasing selections of books.

I am, Sir,
Your's respectfully,
A TRAVELLER.

York Hotel, Bridge Street,
Black Friars.

and to other respectable and opulent places.

P. S. I am sorry to be informed that neither Inverness, Banff, or Peterhead, possess such institutions, particularly the first, which presents such an abundant number of objects to the antiquarian, and is surrounded by, and contains, so many gentlemen of distinguished liberality, and ingenuity; at this place the northern meeting was established for the avowed purpose o promoting intercourse amongst distant families, but how much more might be effected of general amelioration and comfort, by the establishment of a Literary and Antiquarian Society, in which subjects connected with general improvement might be discussed, and books in chemistry, agriculture, and other more immediately useful parts of knowledge, collected.

Notice

XII.

Notice of certain Instances of wasteful Negligence in some Fisheries in the North. By an ENQUIRER.

To Mr. NICHOLSON.

SIR, *London, Oct. 10, 1805.*

IT is mentioned in the Statistical Reports of Banff and Peterhead, that the fishermen there never think of carrying their fish along the coast southward, which they might do, to Leith, in 24 hours, or with a good brisk wind to Berwick-upon-Tweed, or even Newcastle-upon-Tyne: but when their respective towns are supplied *they throw the remainder upon the dunghills for manure!* this was positively affirmed to me as a truth, by a gentleman of great respectability of Aberdeen.—At Arbroath another custom equally as extravagant in its kind prevails, and of which I have been a witness: the crab fishery there is so productive, that after boiling them, *the bodies of the crabs are thrown away,* and the *large claws only* brought to table! Ought not such amazing waste to be remedied? *

Instances of wasteful negligence in some fisheries.

Your's respectfully,

An ENQUIRER.

XIII.

On Bile. By M. THENARD †.

BILE has been commonly considered as a saponaceous liquor charged with albumen: but it has been found, upon closer investigation, to present phenomena which cannot be accounted for

Bile considered as a soap with albumen.

* *Qu.* What may the value of manure procured from fish at these places, compared with the price of the article at the neighbouring markets, subject to the deduction of carriage (coastwise), and the effect of a rival supply from nearer parts of the coast?—The facts which would solve this question, would shew whether the fishermen neglect their interests in these proceedings.—W. N.

† Bulletin des Sciences, No. 95.

for merely by the presence of these principles: this is more particularly to be observed on submitting it to the action of fire and of acids.

Destructive distillation leaves one-eighth, in which is only one-fifth of soda; and this cannot saponify the oil.

Bile, if distilled to dryness, leaves a residuum equal to $\frac{1}{8}$th of its original weight; from 100 parts of which calcined is obtained a carbonaceous matter, comprising several kinds of salt; as marine salt, phosphate of soda, sulphate of soda, phosphate of lime, oxide of iron, and four parts of soda. Bile therefore contains no more than $\frac{2}{100}$ parts of its weight of soda.

So small a portion of alkali would not be sufficient of itself to dissolve that quantity of oil which is known to exist in bile: a fair presumption may therefore be entertained that this liquor contains some other property to supply the absence of alkali. This conjecture increases to strong probability, if not to absolute certainty, in attending to the action of acids on bile.

Acid separates oil and albumen from bile; the clear fluid is bitter, and affords by evap. a residuum.

If a few drops of acid be mixed with bile, a liquor of a reddish tint is obtained, which stains paper of a bright yellow. In this experiment little or no precipitation is perceived; but on the addition of more acid, it takes place abundantly: the matter deposited consists of albumen joined with a very small portion of oil, not at all correspondent to the quantity of these substances to be found united in pure bile. The liquor remaining after filtration is of an extremely bitter taste, and leaves on evaporation a residuum equal to what is obtained from a like quantity of bile in its original state.

The oil with an alkali and albumen is not bile.

On dissolving the oil, which had been previously obtained from bile, in alkali, and adding to the ley produced, a portion of albumen, a combination took place which was decomposed by the most feeble acids, and from which vinegar precipitated all the oil. This combination, therefore, was not bile; consequently bile consists not merely of albumen, oil, and soda; and this is the reason why soluble salts, barites, strontian, lime, and several metallic dissolvents, make no impression upon bile.

Bile contains a peculiar matter;

No longer doubting that there existed in bile a matter peculiar to itself, I endeavoured to separate it; and after a few trials, I succeeded, by means of a combination of acetous acid with lead.

Acetite of lead precip. the oil and alb. The liquor by evap.

On pouring into bile acetite with a slight excess of oxide of lead (that is, acetite of common lead boiled with about the 6th part of its own weight of litharge deprived of its carbonic acid)

acid) the whole of the albumen and oil were precipitated; the liquor being filtered, the oxide of lead and acetite were separated from it by means of sulphurated hidrogen; and by evaporation, after having again filtered the liquor, a substance was obtained whose flavour was at once saccharine and acrid, somewhat similar to the juice of certain kinds of liquorice. But as this substance was still supposed to be charged with the salts of the bile, changed into acetite, by the acetite of lead, it was precipitated with acetite super-saturated with oxide of lead, containing one part of the quantity of acid found in common lead; the precipitate was dissolved in vinegar, to free it from the sulphurated hidrogen, filtered, and again evaporated; by which means the matter was obtained in its greatest purity.

gave a substance which when pure

Its principal qualities are:

had the peculiar qualities here enumerated.

1. Being soluble in water, and in alcohol, slightly deliquescent.

2. It is not precipitated by acetite of common lead; but is entirely so by the saturated acetite of lead, which precipitate is soluble in acetite of soda.

3. It will not ferment with yeast; will give no ammonia by distillation; and is not affected by the presence of nutgall.

4. It dissolves the oily matter of bile: but to facilitate this operation, it is necessary to dissolve the two matters together in alcohol, evaporate, and wash the residuum in water. One part of the saccharine and acrid substance dissolves only three-fourths of the oily matter. Now, as these matters are nearly in equal proportions in bile itself, it must be admitted that soda contributes towards the complete dissolution of the oil; nevertheless acids scarcely, if at all, affect it.

In reflecting on the above experiment and its results, I concluded that bile was a triple compound of a little soda and much oily and saccharine matter; that acids decomposed it but in part; in other words, that it was capable of containing an excess of acid without having its portion of soda neutralized. I therefore calcined bile that had been acidulated with sulphuric, muriatic, and other acids, and found in each case the soda left in the calx: it is therefore very probable that the saccharine matter, in conjunction with the oil, decomposed a certain quantity of marine salt, and destroyed the power of the acid.

Bile consists of little soda and much oily and saccharine matter, &c.

It

Determination of component parts of bile.

It would have been of little service to describe the constituent parts of bile, had their proportions been left unascertained; I have therefore endeavoured to determine them in the following analysis:

Analysis.

By means of nitric acid, I separated the animal substance, which is supposed to be albumen, with a very small portion of oil: this being soluble in alcohol and the other not, it was easy to ascertain the weight of each. I then precipitated all the oily matter, with acetite and a small excess of oxide of lead: this precipitate being mixed with the metallic oxide, I dissolved it in weak nitric acid; after filtering the liquor, I deprived it of the lead which remained, by means of sulphurated hydrogen; and by evaporation, I obtained the peculiar substance, mixed, indeed with the salts of the bile, which had mostly undergone a change by the acetite of lead, and whose weight had noted.

I ascertained the quantity of soda by calcining 100 parts extracted from bile, and comparing with much care how much the residuum would imbibe of acid at 16°, with the quantity imbibed by pure soda. I also, by means not necessary to state here, obtained the quantity of each of the other salts contained in bile; from all which experiments, made with the utmost care, I conclude that 800 parts of the bile of an ox contain—

Numerical result.

Water - - -	700 Parts
Oily matter - - -	43
Particular substance - -	41
Animal substance - -	4
Soda - - -	4
Marine salt - - -	3.2
Sulphate of soda - -	0.8
Phosphate of soda - -	2
Phosphate of lime - -	1.2
Oxide of iron - - -	0.5
	799.7

N. B. This calculation is $\frac{3}{10}$ deficient of the given quantity.

Bile forms an interesting subject for a number of other researches: the varieties to be found in the several species of animals, and which a multitude of circumstances, particularly a morbid affection of the organ which secretes it, may modify;

5 the

the calculi which are there formed, and are of a peculiar nature; the oleaginous and animal substances; and that particular matter, differing from all others hitherto known; will not fail to excite a lively interest, and are the subject of several other Papers which I purpose shortly to bring before the public.

XIV.

Quotation from Sir GEORGE STAUNTON'S *Embassy, containing a Description of Fire Works unknown in Europe. Proposed by a Correspondent with a View to obtain Explanation of the Means by which they were produced.*

To Mr. NICHOLSON.

SIR,

I presume to think it will accord with the general aim of your excellent collection to insert the following quotation; and I indulge the hope that your compliance with my request for that purpose may produce an explanation from some of your ingenious correspondents. Introduction.

I am Sir,

Your constant reader,

P. M.

"After the ballets, Fire-works were played off; and even in the day-time had a striking effect. Some of the contrivances were new to the English spectators. Out of a large box, among other instances, lifted up to a considerable height, and the bottom falling out as if it were by accident, came down a multitude of paper lanterns, folded flat as they issued from the box, but unfolding themselves from one another by degrees. As each lantern assumed a regular form, a light was suddenly perceived of a beautifully coloured flame, burning brightly within it; leaving doubtful by what delusion of the sight those lanterns appeared, or by what property of combustible materials they became thus lighted, without any communication from the outside to produce the flame within. This devolution and development were several times repeated, with a difference Remarkable fire-works of the Chinese.

of figure every time, as well as of the colours, with which the Chinese seem to have the art of clothing fire at pleasure. On each side of the large box, was a correspondence of smaller boxes, which opened in like manner, and let down a kind of net work of fire, with divisions of various forms, which shone like burnished copper, and flashed like lightning at every impulse of the wind. The whole ended with a volcano, or eruption of artificial fire, in the grandest stile."—*See Staunton's Embassy to China, Volume* III. *page* 73.

XV.

On the Carbonate of Potash. By M. STEINACHER *.

Carbonate of potash requires to be formed by passing the gas through a cold alkaline solution till it crystallizes, and not by evaporation.

CHEMISTS know that carbonate of potash well saturated, so as to effloresce, can only be formed by making pass through a cold alkaline solution a quantity of carbonic acid sufficient to cause a spontaneous crystallization. For on stopping the disengagement of gas at the moment when the earth of the alkali appears to be deposited, and evaporating the liquor by a mild heat, there are only laminated crystals obtained, which soon deliquesce.

The gentle heat of Curaudeau does not succeed.

Alkaline ley warmed by heat of tan, according to the method of M. Curaudeau, does not succeed any better in forming a well saturated alkali by evaporation. I have experienced that its crystals grow moist, and the author himself acknowledges a slight deliquescence.

Welter's apparatus is too complicated; and so is that of Pelletier.

It is generally agreed, that the apparatus of Welter, described in the twenty-seventh Volume of the Annales de Chimie, is too complicated, and that of M. Pelletier has been adopted in its place in almost all laboratories, with some alterations in the disposition of the first bottle, to which is fixed a tube with a double or triple perpendicular curvature, for the introduction of the acid, or a long pipe of glass terminated in a tunnel. The chalk mixed with water to the consistence of thin soup, is poured by degrees into this pipe, which is stopped by a glass rod accordingly, and the gas is forced to traverse the bottles containing the alkaline ley.

* From the Annales de Chimie, Tom. LV.

This

This method is in my opinion attended with much inconvenience; for when a tube with many perpendicular flexures is used, it must be charged with a column of the fluid sufficient to counterbalance the pressure of the carbonic acid gas, and consequently to give an elevation which exposes it to be easily broken; and when a long horizontal pipe of glass is employed, it often happens that the chalk is exploded into the air, when the piston is opened. The reason stated.

Another method appears to be more simple and commodious than this, which is something like that of M. Brugnatelli, but the Italian chemist has not published the details of his method, without which it is impossible to be followed. Simpler method not yet described.

A kilogram of chalk in powder is to be put into a bottle with two or three necks, capable of containing 12 kilograms of water; on this is to be poured a litre of a mixture of one and a half kilogram of vitriolic acid, with nine kilograms of spring water: the gas is expelled, and a crust of sulphate is formed at the surface of the calcareous carbonate. At the end of two hours all the rest of the acid water is to be added, and the bottle stopped quickly. Bubbles of the gas will be rapidly disengaged, but they will by degrees be discharged more slowly, and continue to be so moderately for twenty-four hours; then the mixture is to be stirred with an iron rod, and the gas will continue to be developed for 24 hours more, with little interruption.

I found the term of the effervescence prolonged by the resistance which was opposed to the action of the sulphuric acid on the chalk by the density which the combination necessarily acquired; which density the tendency of the sulphuric acid to augment the solubility of the sulphate of lime, fixed to proper limits.

As my apparatus, with the exception of the first bottle, is the same of that of M. Pelletier described in the fifteenth Volume of the Annales, I shall not speak of it, but only make the following remarks:

1. Pelletier was wrong in neglecting to use an intermediate bottle half filled with water: which is very necessary to separate the sulphuric acid which the gas always brings over.

2. The tubes of an inch diameter, being too difficult to be bent, may be replaced by others of seven or eight lines aperture, which will do equally well.

3. The

3. The alkaline ſolution, made by two lb. alkali to three lb. water, cryſtallizes too quick, and before the precipitation of the ſilex.

4. The doſes of alkali and water moſt favourable to a regular cryſtallization, at the temperature of from 5 to 10 above O of Reaumur, are one part of diſtilled water, and half a part of purified potaſh.

If the reſults of my experiments ſhall improve the preparation of carbonate of potaſh, and if the diſpoſition of my apparatus prevents the neceſſity of continually watching its direction, by procuring without trouble that gentle and continued preſſure, of which Pelletier perceived the efficacy for the ſaturation of the alkali, I have reaſon to think that the true friends of chemiſtry, I mean thoſe who practiſe it, will conſider my obſervations with indulgence.

XVI.

*Method of preparing a luminous Bottle, which long preſerves its Effect.**

A luminous bottle. Put a ſmall piece of phoſphorus in a long phial: pour on it boiling oil. The fluid will give light in the vacuous ſpace whenever the cork is pulled out.

IT is eaſy to prepare a bottle which ſhall give ſufficient light during the night to admit of the hour being eaſily ſeen on the dial of a watch, as well as other objects, by the following means.

A phial of clear white glaſs, of a long form, ſhould be choſen, and ſome fine olive oil ſhould be heated to ebullition in another veſſel: A bit of phoſphorus, of the ſize of a pea, ſhould be thrown into the phial, and the boiling oil ſhould then be carefully poured over it, till the phial is one-third filled: The phial ſhould then be carefully corked; and, when it is to be uſed, it ſhould be unſtopped, to admit the external air, and cloſed again: The empty ſpace of the phial will then appear luminous, and give as much light as a dull ordinary lamp. Each time that the light diſappears, on removing the ſtopper it will inſtantly re-appear. It is proper to obſerve, that in cold weather it will be neceſſary to warm the bottle for a little while in the hands before the ſtopper is removed, without which precaution it would not yield any light.

* Sonini's Journal.

A phial

A phial thus prepared may be used every night for six months; there is no danger of fire from it, and its cost is very small.

XVII.

Analysis of the Magnesian Earth of Baudissero in Canavais (in the Department of the Loire,) known by the Name of Porcelain Earth, and hitherto considered as a Clay. By M. GIOBERT.*

THE earth of Baudissero, known by the name of porcelain earth, has been hitherto considered as one of the purest argillaceous earths known in the history of fossils, and is arranged in our cabinets of minerals as native alumine. **Porcelain earth considered as native alumine.**

In a manufacture of stone-ware pottery, which has been established at Vineuf, this earth has been used for a long time, as a clay of extraordinary purity. The celebrated Macquer, and with him Baume, to whom specimens of this earth were sent from the above manufacture, pronounces positively that it was a clay of superior quality to that which they used in the manufactory of porcelain at Sevres. **Used as such in the stone-ware pottery at Vineuf.** **Various chemists adopted the same conclusion.**

Doctor Gioanetti continued to use it in the manufacture of his fine porcelain at the same Vineuf; and he engaged in, if not an analysis, at least some experiments on this earth, to ascertain more precisely the proportions in it between silex and earth, which he believed to be pure alumen. These experiments convinced Doctor Gioanetti, that, with the exception of a little carbonic acid which he found in it, the earth of Baudissero was an alumen almost perfectly pure, or at least the purest that he had ever met with.

This chemist, when I made enquiries of him relative to this earth, assured me frequently, that picked pieces yielded him sometimes ninety *per cent.* of alumen, including a little carbonic acid, and that in the gross it yielded constantly at least 80. **The alumine was estimated at 80 per cent.**

On the perusal of the mineralogical description of the mountains of Canavais by the Chevalier Napion, it will be found that this estimable mineralogist has not hesitated to declare the **Other authority.**

* Journal de Physique, LX.

earth

earth of Baudissero to be the most pure alumen ever found in Piedmont; and again in his elements of mineralogy, he mentions the earth of Baudissero as native alumen.

Contrary to these assertions this native earth contains no alumine at all.

Facts so positively asserted by scientific men so estimable as Maquer, Baume, and our colleagues Gioanetti and Napion, admitted no doubt of the nature of this earth; to which authorities might be added the success with which Gioanetti constantly used it in his porcelain manufacture.

Among a number of researches which I made relative to the artificial fabrication of sulphate of alumen, I employed myself on this earth, and to my great surprize found that the earth of Baudissero not only was not pure alumen, but did not even contain an atom of it.

Immense quantities of sulphuret of iron at Baudissero.

The town of Baudissero is situated at least three leagues from Ivree and from Brozo, this last village, as celebrated for its iron mines as for the manner in which they are wrought, contains in a mountain, among other minerals, an inexhaustible mass of sulphuret of iron of a remarkable purity, where there is established a manufacture of sulphate of iron by the combustion of the sulphur.

Efflorescence of the neighbouring blocks of stone by sulphureous vapours.

On inspecting this manufacture last year, I was struck with the singularly powerful action, which the sulphureous acid, formed by the combustion of the sulphur, (and of which a part expanded itself to neighbouring places,) exercised on the great blocks of stone.

These stones were a sort of granite schistus; and the sulphureous acid attacked them so forcibly that it made them exfoliate, and at last reduced them to a species of efflorescence, or white powder evidently saline, in which its astringent taste announced sulphate of alumen.

Probability that sulphate of alumine or alum might be advantageously made from the porcelain earth.

This circumstance made me think that if a good argil was exposed to the action of the acid it would be alumenated; and the earth of Baudissero, which I believed to be almost pure alumen, being at such an inconsiderable distance, made me conceive the hope of being able to establish with economy at Piedmont, a manufacture of artificial sulphate of alumen.

Very promising local advantages.

The idea of this establishment appeared to me to be so much the more fortunate as there was at the foot of the same mountain which contained the pyrites, a great turbary, which extended almost as far as Chinsella, that is to say, almost to Baudissero, and which might furnish fuel at a very small expense;

pense; and it seemed to me that nature, in placing at one side an inexhaustible mine of sulphur, and at the other inexhaustible masses of the proper earth of an extremely rare purity, and between them an abundant supply of fuel, of the sort most proper for this kind of work, had done its utmost in favour of the establishment I intended.

There only remained to make some experiments with a view to ascertain the most profitable way of proceeding; and to examine principally if the iron which is united to the sulphur in the pyrites, would not be injurious to the sulphate of alumen obtained. Experiments preparatory.

With this design I began by examining the action of the earth of Baudissero on the sulphate of iron, and the quantity of the earth necessary for the decomposition of a given weight of sulphur.

In the different experiments the sulphate of iron dissolved in water, and boiled with this earth in different proportions became evidently decomposed after boiling for less than a quarter of an hour; the iron was precipitated of a blackish grey, while the solution was colourless, and ammonia dropped into it formed only a very white precipitate, which did not announce much iron: I filtered the liquor, of which one part was mixed with a little potash, and placed so as to crystalize; and to ascertain whether there was any potash in the earth of Baudissero, I set another part to crystalize without any alkali. The earth of Baudissero decomposed sulphate of iron in the humid way. A little potash was added and the liquor set to crystallize.

I observed that the liquors crystallized immediately after becoming cold; but in the place of octahedrons, I found the most perfect, the most beautiful, and purest crystals of sulphate of magnesia. It gave sulphate of magnesia and not allum.

The liquor which remained produced, on a new evaporation, the same pure crystals of sulphate of magnesia; and did the same on successive evaporations and crystalizations to the last drop of the liquor. In this manner was a natural alumen transformed entirely into magnesia, and at the same instant magnesia became at once an excellent porcelain earth. If examples of this kind should multiply, the necessity of chemical analysis for the knowledge of fossils will become more and more manifest, and less reliance will be learned to be placed on their external and physical characters, which at present seems to me to be too much abused. All the crystals were of this kind, and hence magnesia is an excellent porcelain earth.

The

More careful examination of the native earth.

The above unexpected results engaged me to make a more careful examination of the earth of Baudissero, and which is the object of this memoir.

At the time when I found that the supposed alumen of Baudissero in Canavais, was really a magnesian earth, I knew of no other example of an earth truly magnesian, but that of the earth of Salinelle, or of Sommieres, which Berard had made known (*Annales de Chimie, Tome* XXXIX. p. 65.)

Other specimens of native magnesia.

In this magnesian earth there is no mixture of any other earth except silex, and that in a very small proportion, of which fact there are but few examples. But on receiving the twelfth volume of Brochant's Mineralogy, I found that the discovery of a magnesian earth was announced in it, which is the native carbonate of magnesia found by Doctor Mitchel at Roubschitz in Moravia. From the analysis which he made of it, and which is mentioned by Brochant, we are assured that the native carbonate of magnesia of Moravia is composed only of magnesia and carbonic acid in almost equal parts; but the yellowish grey colour spotted with black, which Doctor Mitchel gives to this earth, seems sufficiently to indicate the existence of some other constituent parts. On comparing the characters and nature of the magnesian earth of Baudissero, it will be easy to perceive the differences which distinguish it from the other preceding magnesian earths.

Local situation of the earth of Baudissero.

The magnesian earth of Baudissero is found disposed in a vein in a steatite rock, of which the mountain is composed that encloses it. It is accompanied by an horn-stone, sometimes of a transparent pale colour, sometimes, when its decomposition commences, of a white almost opaque. In this state the horn-stone does not appear to be that of which Doctor Bonvoisin has given the description and analysis, under the name of the Hydrophane of Piedmont.

It is found in masses, lumps, or fragments.

Our magnesian earth appears in masses, sometimes in roundish lumps (mamelonnés) and sometimes in fragments more or less large; the lumps and fragments are sometimes, but rarely, tuberculose.

Beautifully white.

This earth is of the most beautiful white, in which it differs from that of Moravia, of which the colour is a yellowish grey spotted with black, and from that of Salinelle, or of Sommieres, which is of a chocolate colour.

The

The hardneſs of this earth is variable, ſometimes it is ſoft, in which ſtate I ſhall call it the *earthy* ſort, and ſome pieces of it have a conſiderable hardneſs; as in all my experiments I tried them comparatively, I ſhall name this laſt variety the *ſtony* kind, to diſtinguiſh it from the preceding.

Soft or hard ſo much as to ſcratch ſteel.

The ſtony variety is ſcratched by ſteel, ſometimes, on the contrary, it is hard enough to ſcratch ſteel. It can be eaſily reduced to powder; but with much difficulty to very fine powder, and this only takes place after long trituration in a mortar of porphyry. Its hardneſs neither increaſes nor diminiſhes by the action of the air; in this reſpect it differs from the magneſia of Moravia, which is very friable, and from that of Salinelle, which is ſoft in its bed, and only grows hard on exſiccation in the air.

Pulverable, tho' not finely, permanent in the air.

The fracture of this variety is conchoidal and unequal.

Fracture conchoidal.

Its ſurface is dull; ſometimes, but very rarely, ſhining ſpots appear. It is conſtantly perfectly opaque, and moderately heavy; its ſpecific gravity is variable.

Dull, opake, moderately heavy.

It is a little unctuous to the touch in the friable and earthy ſort, and but very little ſo in the ſtony variety.

Slightly unctuous and adheſive.

It ſenſibly adheres to the tongue, though not much; it acquires this property in a conſiderable degree when it is moderately warmed at the fire.

Plunged in water, the ſtony variety does not abſorb it at all; the friable ſort abſorbs it greedily, and with an hiſſing, but the mixture does not grow hot.

The ſoft ſpecimens abſorb water and mix like clay.

The friable ſpecies mixes with water to a conſiderable degree, in the ſame manner as clay; the fine particles of this earth, like thoſe of clay, continue a long time ſuſpended in water, with this difference from thoſe of clay, that they do not unite together. Urged by the blow-pipe, on a cianite cryſtal, it is infuſible.

Are not fuſible by the blow-pipe.

Treated in a maſs, on the fire in a crucible, eſpecially in a red hot crucible, it ſoon decrepitates, and divides into thick ſcaly pieces, which leap out of the crucible; this does not happen if it is heated by degrees and moderately.

If it is reduced to a fine powder, and then traited on the fire, as ſoon as the bottom of the crucible begins to grow red hot, this earth boils for a ſhort time, and ſeems to unite together, as if moderately moiſtened.

But apparently ſo in a crucible.

An

With loss of one seventh.

An hundred parts of this earth treated in this manner, until the boiling ceases, after an hour of incondescence, became reduced to 85, and 0,40. The earth calcined in this manner throws out that blueish light which has been observed from common magnesia.

Giving out carbonic acid.

If the calcination is made in a retort of earthen ware, to which a syphon is adapted, and plunged into a bottle filled with lime water, there is formed in the bottle carbonate of lime; so that the diminution of weight is partly due to the disengagement of carbonic acid.

It contains a minute quantity of sulphate of lime.

A thousand grains of this earth in fine powder were boiled in six pounds of distilled water. The liquor being filtered, and then essayed by various reagents, presented the following results.

With the solutions of the acetate, nitrate, and muriate of barytes, the mixture became troubled almost instantly, and formed a sediment of sulphate of barytes, but in a very small quantity.

The oxalate of ammonia formed oxalate of lime with it, but also in a very small portion.

These experiments repeated different times on the earth both of the stony and friable varieties, constantly gave the same results.

Lime and sulphuric acid, or sulphate of lime, is therefore, although in a small degree, a constituent part of the earth of Baudissero both in the stony and soft state.

A minute portion of muriatic acid seems to be present in the stony variety.

The nitrate of silver formed a precipitate equally with both sorts; but remarkable differences were observed between its effects in the water from the stony species, and on that from the soft variety; with the latter it formed a precipitate, which collected in a powder at the bottom of the glass; but with the water from the stony kind, besides the precipitate, filaments were produced constantly, which indicated the presence of muriatic acid. Many times the "sulphuric acid*" was first removed by the acetate of barytes, and after filtration, on being treated with the nitrate of silver, still formed a precipitate of muriate of silver.

* It is not clear what the removal of the *sulphuric acid* mentioned here had to do with making the appearance of the *muriate of silver* seem extraordinary; perhaps it is an error in the original; the translation is literal and correct. B.

The infufion of the ftony fort afforded differences from the other, with ammonia alfo; this reagent did not ever trouble the infufion of the friable fpecies, but always troubled, though flightly, the infufion of the ftony variety.

It follows from thefe obfervations that, befides the fulphate of lime which both kinds of the earth of Baudiffero contained, the ftony variety held in union muriatic acid, perhaps in combination, partly with the lime, which there was not fufficient fulphuric acid to faturate, and partly united to another earth, which was not lime, fince its folution permitted itfelf to be decompofed by ammonia; and it will appear that this earth was magnefia.

The fulphuric, nitric, and muriatic acids attack this earth, when it is well divided into an extremely fine powder.

The ancient mineral acids attack this native earth.

Their action however is but little apparent, but on the leaft addition of heat it becomes ftrongly marked. Very fmall bubbles of gas, which rife from the bottom of the liquor, a flight white fcum which forms itfelf at the furface, and a fmall hiffing, fhews plainly that there is a difengagement of an aeriform fluid or effervefcence.

When the earth has been previoufly calcined in the fire, their action is very different. There is not, as may be forefeen, any effervefcence; but the mixture grows hot, to that degree that a true ebullition enfues; in fome minutes the mixture affumes a folid form, caufed by a kind of jelly produced by it.

With great force if previoufly calcined.

The acid which has the greateft action on it is the muriatic acid, and after this the nitric, and the fulphuric acid after both. This laft however does not diffolve without much difficulty the whole of the foluble part, and that after a long continued ebullition.

The muriatic acid acts more ftrongly.

The folution made in the clofed veffels difpofed fo as that the gas may be received, forms with lime-water carbonate of lime, which confirms the difengagement of a little carbonic acid before indicated by the calcination of this earth in the fire.

The folutions of this earth in the acids are perfectly colourlefs.

The pruffiate of lime or the oxalate of ammonia do not at all trouble them.

Ammonia forms with them an abundant precipitate.

Ammonia precipitates the folution as does likewife the carbonate of potafh.

The

The common unsaturated carbonate of potash forms also a precipitate with them.

When this carbonate ceases to trouble the liquor, and that, after being left to settle and being filtered, the clear liquor is submitted to ebullition, it becomes troubled again and throws down a second time an earthy precipitate.

But not if saturated with c. acid.

Finally, if instead of the unsaturated carbonate, the carbonate of potash, well saturated with acid is used, there is not the least precipitate formed.

The earth is pure magnesia.

Experiments, which I will relate, shew not only that the earth dissolved by the acids is magnesia, but that there is not mixed with it the least particle of lime, which can be discovered by the oxalate of ammonia, that there is no trace of alumen in it, which the saturated carbonate of potash precipitates, and does not again dissolve; that it does not contain the least oxide of iron, that can be indicated by the prussiate of lime; and finally, that it is magnesia perfectly pure.

As is shewn by the sulphate.

This result is farther confirmed by the sulphate of magnesia, which the crystallization of the solution of this earth in sulphuric acid yields exclusively.

Insoluble residue or silex about one 6th.

The acids in dissolving this earth leaves a residue, the quantity of which seems to be variable; that which the sulphuric acid leaves is constantly more than what is left by the muriatic or nitric acids. An hundred and twenty grains of this earth, after being well lixiviated in pure water, left a residue of which the weight, in the different experiments which I made, did not ever exceed 17 grains, and never was less than 14. The stony variety was that which in general gave the most of this insoluble residue. Many experiments, which I have made, and which it would be useless to repeat here, have convinced me that this residue is perfectly pure silex.

Component parts recapitulated.

The earth of Baudissero, from the preceding experiments, consists entirely of magnesia with a little carbonic acid, a small quantity of silex, and a very minute portion of sulphate of lime, with, in the stony variety, some traces of muriate of magnesia.

(To be concluded in the Supplement.)

Experiments

XVIII.

Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr. HARDING'S *lately discovered Star. By* WILLIAM HERSCHEL, *L. L D. F. R. S. Abridged from the Phil. Trans.* 1805.

THE discovery of Mr. Harding having added a moving celestial body to the list of those that were known before, Dr. Herschel was desirous of ascertaining its magnitude: and as in the observations which it was necessary to make he intended chiefly to use a ten-feet reflector, it appeared to be a desideratum highly worthy of investigation to determine how small a diameter of an object might be seen by this instrument. It is known that a very thin line may be perceived, and that objects may be seen when they subtend a very small angle; but the case he wanted to determine related to a visible disk; a round, well defined appearance, which might without hesitation be affirmed to be circular, if not spherical.

Enquiry as to the smallest angle under which the eye by a telescope can determine the figure of an object.

In April of the year 1774, the Doctor determined a similar question relating to the natural eye: and found that a square area could not be distinguished from an equal circular one till the diameter of the latter came to subtend an angle of 2′ 17″.

The author's unassisted eye cannot distinguish a smaller circle than of 2′ 17″ from an equal square.

He did not think it right to apply the same conclusions to a telescopic view of an object, and therefore had recourse to the a series of experiments.

The first course of experiment, was made with the heads of pins deprived of their polish by tarnishing them in the flame of a candle. The diameters of the heads were measured by a microscopic projection, with a magnifying power of 80. These measures were so exact, than when repeated they seldom differed more than a few ten thousandths part of an inch from each other. The focal length of the mirror on Arcturus is 119,64 inches, but on these objects 125,9, and the distance was measured with deal rods.

Experiments with the telescope: the objects were pin's heads.

And the result of this experiment was that an object having a diameter ,0425 could be easily seen in the author's ten-foot telescope to be a round body, when the magnified angle under which

which it appeared was 2′ 18″,9, and that with a high power (522) a part of it, subtending an angle of 0″,364 may be conveniently perceived.

When the purpose of this experiment, was considered, the result was not found sufficient to answer the intention; for as the size of the object required the use of a low power, a doubt arose whether the instrument would be equally distinct when a higher should be required. To resolve this question, it was necessary either to remove my objects to a greater distance, or to make them smaller.

With globules of sealing-wax.

2. Small globules of sealing-wax were therefore made by dipping the point of a fine needle, a little heated, into it, which took up a small globule. To prevent seeing them at a distance in a different aspect from that in which they were measured under the microscope, the needles were fixed with sealing-wax on small slips of cards before the measures were taken.

By this experiment it was found, that with a globule so small as ,00763 of a substance not reflecting much light, the magnified angle must be between 4 and 5 minutes before we can see it round. But it also shewed that a telescope with a sufficient power (522) will show the disk of a faint object when the angle it subtends as the neked eye is no more than 0″,653.

Globules of silver.

The third experiment was made with globules of silver. As the objects made of sealing-wax, on account of their colour, did not appear to be fairly selected for these investigations, a set of silver ones were made. They were formed by running end of silver wires, the 305th and 340th part of an inch diameter, into the flame of a candle.

By this experiment it was found that the telescope acted very well with a high power (522), and will show an object subtending only 0″,484 so large that it may be divided into quarters of its diameter,

Experiments with other globules, and with silver at greater distances, &c.

The fourth experiment was made with globules of pitch, bee's-wax, and brimstone, and did not prove so generally advantageous as those with silver which reflected more light.

And a fifth and sixth experiment was made with the silver globules at greater distances; and by illuminations at night by the flame of a lamp of which for brevity the particulars are here omitted.

The

The author then proceeded to make direct obſervations on the ſpurious diameters of celeſtial bodies, from which he deduces the following reſults:*

(1.) As the diameters of fixed ſtars are undoubtedly ſpurious, it follows that, with the ſtars, the ſpurious diameters are larger than the real ones, which are too ſmall to be ſeen.

Spurious diameters of ſtars greater than real.

(2.) From many eſtimations of the ſpurious diameters of the ſtars † it follows, not only that they are of different ſizes, but alſo that under the ſame circumſtances, their dimenſions are of a permanent nature.

Sizes are the ſame in like circumſtances;

(3.) By this and many other obſervations it appears, that the ſpurious diameters of the ſtars are differently coloured, and that theſe colours are permanent when circumſtances are the ſame.

and colours.

(4.) By many obſervations, a number of inſtances of which may be ſeen in Dr. Herſchel's catalogues of double ſtars, their ſpurious diameters are leſſened by increaſing the magnifying power, and increaſe when the power is lowered.

They are leſs with high powers;

(5.) It is alſo proved by the ſame obſervations, that the increaſe and decreaſe of the ſpurious diameters, is not inverſely as the increaſe and decreaſe of the magnifying power, but in a much leſs ratio.

but not proportionally.

(6.) The magnifying power acts unequally on ſpurious diameters of different magnitudes; leſs on the large diameters, and more on the ſmall ones.

Small ſtars are moſt enlarged.

(7.) When the aperture of the teleſcope is leſſened, it will occaſion an increaſe of the ſpurious diameters, and when increaſed will reduce them.

Leſs aperture cauſes greater ſp. diameter; and ſmall ſtars are moſt affected by this change.

(8.) It alſo ſhows that the increaſe and decreaſe of the unequal ſpurious diameters, by an alteration of the aperture of the teleſcope, is not proportional to the diameters of the ſtars:

(9.) But that this alteration acts more upon ſmall ſpurious diameters, and leſs upon large ones.

(10.) From this we find that ſtars, when they are extremely ſmall, loſe their ſpurious diameters, and become nebulous.

Very ſmall ſtars appear nebulous,

* On this ſubject ſee our Journal, Vol. VI. p. 15, and Fig. 1, Plate IV.

† See Catalogue of double Stars. Phil. Tranſ. for 1782, p. 115; and for 1785, p. 40.

(11.) Many

Other causes affect the spurious diameters.

(11.) Many other causes will have an influence on the apparent diameter of the spurious disks of the stars, such as the goodness of the specula and lenses; but they are so far within the reach of our knowledge, that with a proper regard to them, the conclusion he has drawn in Rem. (2.) "that under the same circumstances their dimensions are permanent," will still remain good.

Similar experiments were made on the spurious diameters of terrestrial objects, namely silver globules, which afforded the following results:

Spurious disks of globules are smaller than the real disks.

(1.) The terrestrial spurious disks of globules are less than the real disks; whereas we have seen, in Remark (1.) of the celestial spurious disks, that these are larger than the real ones.*

Larger magnitudes give larger sp. disks;

(2.) The less globule has the largest spurious disk. This agrees with the spurious disks of celestial objects: the stars of the first, second, and third magnitude, having a larger spurious disk than those that are of inferior magnitudes.

coloured like the celestial;

(3.) With respect to colours, the terrestrial also agree with the celestial spurious disks.

Less with greater mag. power;

(4.) The spurious diameters of the globules, like the spurious disks of the stars, are proportionally lessened by increasing the magnifying power, and increased when the power is lowered.

But not proportionally.

(5.) When the estimations are compared with the powers, it will also be seen that the increase and decrease of the spurious disks of the globules is not inversely as the powers, but in a much less ratio.

Power acts more on small than large sp. disks,

(6.) The effect of magnifying power is unequally exerted on spurious diameters; and that, as with celestial objects, so with terrestrial, this power acts more on the small spurious disks than on the large ones.

and diminution of aperture;

(7.) The spurious terrestrial disks also resemble those of the stars, by increasing when the aperture is lessened, and decreasing when it is enlarged.

greater on small disks.

(8.) By these experiments it is proved, that the increase and decrease of the diameters occasioned by different apertures is not proportional to the diameters of the spurious disks.

* It appears from the context, that this arises from the terrestrial spurious disks being formed by the small spot of reflected light from the metallic globule, and not from its whole diameter.

5 (9.) Bu

(9.) But that the change of the apertures acts more on the small, and less on the large ones.

(10.) The spurious disks of globules are lost for want of proper illumination, but do not change their magnitude on that account. The brightness of the atmosphere in a fine day is sufficient to produce them; though the illumination of the sun is generally the principal cause of them.

These disks are not changed by diminution of light.

(11.) The diameters of spurious disks are liable to change from various causes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally polished in its surface, or of equal convexity every where, being very seldom perfectly spherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the spurious disks will generally return to its former size.

Globules of mercury were used instead of those of silver, and with the same results.

Mercurial globules.

The spurious terrestrial disks were then measured by comparing them with circles on a tablet; and it was found that a variation in their illumination did not affect their magnitude. It was also found that the rays from the central part of the mirror gave a larger image than those from its circumference. So that when a central aperture of three inches gave an image corresponding with a circle of 0.465 inch, an annular opening from 6.5 to 8.8 inches gave only 0.18 inch for the image: and the experiments were sufficiently varied as to the magnitudes and situations of the apertures to shew that this difference did not arise from more or less light.

Measurement of spurious disks. They may be distinguished from real disks by using first a central, and then an annular aperture. The first enlarges and the second diminishes them.

This property of the mirror serves admirably to distinguish a spurious disk from a real one; and proved to be so on trial with terrestrial and celestial objects.

Trials.

Observations on the Nature and Magnitude of Mr. HARDING'S *lately discovered Star.*

On the day Dr. Herschel received an account of Mr. Harding's new star, which was the 24th of September, he directed his telescope to the calculated place of the new object, and noted all the small stars within a limited compass about it. They were then examined with a distinct high magnifying power; and since no difference in their appearance was

Observations on the planet Juno.

Observations on the planet Juno.

was perceivable, it became necessary to attend to the changes that might happen in the situation of any one of them. They were delineated as in Fig. 1, (Plate XIV.) which is a mere eye-draught, to serve as an elucidation to a description given with it in the journal; and the star marked *k*, was the new object.

Sept. 29. Being the first clear night, he began a regular series of observations: and as the power of determining small angles, and distinctness in showing minute disks, whether spurious or real, of the instrument he used on this occasion, had been sufficiently investigated by the foregoing experiments, there could be no difficulty in the observation, with resources that were then so well understood, and have now been so fully ascertained.

"Mr. Harding's new celestial body precedes the very small star in Fig. 3, between 29 and 33 Piscium, and is a little larger than that star; it is marked A. *f g h* are taken from Fig. 1. I suppose *g* to be of about the 9th magnitude, so that the new star may be called a small one of the 8th."

With his ten-feet reflector, power 496.3, he viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger star.

By way of putting this to a trial, he changed the power to 879,4, but could not find that it magnified the new one more than it did the stars *g* and *h*.

"I cannot perceive any disk; its apparent magnitude with this power is greater than that of the star *g*, and also a very little greater than that of *h*; but in the finder, and the night-glass *g* is considerably smaller than the new star, and *h* is also a very little smaller."

He compared it now with a star which in the finder appeared to be a very little larger; and in the telescope with 879,4 the apparent magnitude of this star was also larger than that of the new one.

"As far as I can judge without seeing the asteroids of Mr. Piazzi and Dr. Olbers at the same time with Mr. Harding's, the last must be at least as small as the smallest of the former, which is that of Dr. Olbers."

"The star *k*, Fig. 1, observed Sept. 24, is wanting, and was therefore the object I was in search of, which by computation must have been that day in the place where I saw it."

"The new star being now in the meridian with all those to which I am comparing it, and the air at this altitude being very clear, I still find appearances as before described: the new object cannot be distinguished from the stars by magnifying power, so that this celestial body is a true asteroid."

Observations on the planet Juno.

Mr. Bode's stars 19, 25 and 27 Ceti are marked 7m, and by comparing the asteroid, called Juno, with these stars, it has the appearance of a small one of the 8th magnitude.

With regard to the diameter of Juno, the author remarks that had it been half a second, he must have instantly perceived a visible disk. Such a diameter, when he saw it magnified 879,4 times, would have appeared under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have escaped his notice.

Oct. 1. Between flying clouds, the asteroid was seen, which in its true starry form has left the place where it was seen Sept. 29. It has taken the path in which by calculation it was expected to move. This ascertains that no mistake in the star was made when last observed.

Oct 2, 7h. Mr. Harding's asteroid is again removed, but is too low for high powers.

8h 30′. Viewed it now with 220,3 288,4 410,5 496,3 and 879,4. No other disk was visible than that spurious one which such small stars have, and which is not proportionally magnified by power.

With 288,4, the asteroid had a larger spurious disk than a star which was a little less bright, and a smaller spurious disk than another star that was a little more bright.

Oct 5, with 410,5. The situation of the asteroid is now as in Fig. 4. Its disk, which is probably the spurious appearance of stars of that magnitude, was compared with a larger, an equal, and a smaller star. It was less than the spurious disk of the larger, equal to that of the equal, and larger than that of the smaller star. The gradual difference between the three stars is exceedingly small.

"With 496,3, and the air uncommonly pure and calm, I see so well that I am certain the disk, if it be not a spurious one, is less than one of the smallest globules I saw this morning in the tree."

The

Obfervations on the planet Juno.

The diameter of this globule was ,02. It fubtended an angle of 0″,429, and was of fealing-wax; had it been a filver one, it would have been ftill more vifible.

With 879,4. All comparative magnitudes of the afteroid and ftars, remain as with 496,3.

The minute double ftar *q* Ophiuchi * was feen in high perfection, which proves that the air is clear, and the telefcope in good order.

The afteroid being now in the meridian, and the air very pure, the comparative diameter feems a little larger than that of an equal ftar, and its light alfo differs from ftar-light. Its apparent magnitude, however, can hardly be equal to that of the fmalleft globule obferved this morning. This globule meafured ,01358, and at the diftance of 9620,4 inches fubtended an angle of 0″,214.

When the afteroid was viewed with 879,4, more hazinefs was found than an equal ftar would have given: but this the Doctor afcribes to want of light. What he calls an equal ftar, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. Harding's afteroid has continued its retrograde motion. The weather is not clear enough to allow the ufe of high powers.

Oct. 8. If the appearance refembling the fpurious difks of fmall ftars, which I fee with 410,5 in Mr. Harding's afteroid, fhould be a real diameter, its quantity then by eftimation may amount to about 0″,3. This judgment is founded on the facility with which I can fee two globules often viewed for this purpofe.

The angle of the firft is 0″,429, and of the other 0″,214; and the afteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill-defined hazy appearance, but nothing that may be called a difk vifible. When there is a glimpfe of more condenfed light to be feen in the centre, it is fo fmall that it muft be lefs than two-tenths of a fecond.

To decide whether this apparent condenfed light was a real or fpurious difk, he applied different limitations to the aperture of the telefcope, but found that the light of the new ftar was too feeble to permit the ufe of them. From this he concluded

* See Cat. of double Stars, I. 87.

that

that an increafe of light might now be of great ufe, and viewed the afteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of thefe large mirrors is very feldom the fame as that of the air in which they are to act; and till a perfect uniformity takes place no high powers can be ufed. Obfervations on the planet Juno.

The afteroid in the meridian, and the night beautiful. After many repeated comparifons of equal ftars with the afteroid, I think it fhows more of a difk than they do, but it is fo fmall that it cannot amount to fo much as 3-tenths of a fecond, or at leaft to no more.

It is accompanied with rather more nebulofity than ftars of the fame fize.

The night is fo clear, that I cannot fuppofe vifion at this altitude to be lefs perfect on the ftars, than it is on day objects at the diftance of 800 feet in a direction almoft horizontal.

Oct. 11. By comparing the afteroid alternately and often with equal ftars, its difk, if it be a real one, cannot exceed 2, or at moft 3-tenths of a fecond. This eftimation is founded on the comparative readinefs with which every fine day I have feen globules fubtending fuch angles in the fame telefcope, and with the fame magnifying power.

" The afteroid is in the meridian, and in high perfection. I perceive a well defined difk that may amount to 2 or 3-tenths of a fecond; but an equal ftar fhows exactly the fame appearance, and has a difk as well defined and as large as that of the afteroid."

Refult and Application of the Experiments and Obfervations.

We may now proceed to draw a few very ufeful conclufions from the experiments that have been given, and apply them to the obfervations of the ftar difcovered by Mr. Harding; and alfo to the fimilar ftars of Mr. Piazzi and Dr. Olbers. Thefe kind of corollaries may be expreffed as follows.

(1.) A 10-feet reflector will fhew the fpurious or real difks of celeftial and terreftrial objects, when their diameter is ½ of a fecond of a degree; and when every circumftance is favourable, fuch a diameter may be perceived fo diftinctly, that it can be divided by eftimation into two or three parts.

(2.) A

Observations on the planet Juno.

(2.) A disk of ¼ of a second in diameter, whether spurious or real, in order to be seen as a round, well defined body, requires a distinct magnifying power of 5 or 6 hundred, and must be sufficiently bright to bear that power.

(3.) A real disk of half a second in diameter will become so much larger by the application of a magnifying power of 5 or 6 hundred, that it will be easily distinguished from an equal spurious one, the latter not being affected by power in the same proportion as the former.

(4.) The different effects of the inside and outside rays of a mirror, with regard to the appearance of a disk, are a criterion that will show whether it is real or spurious, provided its diameter is more than ¼ of a second.

(5.) When disks, either spurious or real, are less than ¼ of a second in diameter, they cannot be distinguished from each other; because the magnifying power will not be sufficient to make them appear round and well defined.

(6.) The same kinds of experiments are applicable to telescopes of different sorts and sizes, but will give a different result for the quantity which has been stated at ¼ of a second of a degree. This will be more when the instrument is less perfect, and less when it is more so. It will also differ even with the same instrument, according to the clearness of the air, the condition and adjustment of the mirrors, and the practical habits of the observer.

XIX.

Account of some new Improvements on Steam-Engines. By Mr. Arthur Woolf.

Mr. Woolf's improvements in steam-engines.

In our eighth volume, p. 262, we gave a short account of a former improvement made by Mr. Woolf on the steam-engine, founded on a discovery that steam, of any higher temperature than that of boiling water, if allowed to pass into another vessel kept at the same temperature as the steam itself, will expand to as many times its volume, and still be equal to the pressure of the common atmosphere, as the number of pounds which such steam, before being allowed to expand, could maintain on each square inch of a safety-valve exposed

exposed to the atmosphere: for example, that masses or quantities of steam of the expansive force of 20, 30, or 50 pounds the square inch of a common safety-valve, will expand to 20, 30, or 50 times its volume, and still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt.

Mr. Woolf's improvements in steam-engines.

In consequence of this discovery Mr. Woolf was enabled to use his steam twice (if he chose), and with complete effect; nothing more being necessary than to admit high steam, suppose of 40 pounds the square inch, into one cylinder, to work there by its expansive force, and then to allow the same steam to pass into, and expand itself in, another cylinder of forty times the size of the first, there to work by condensation in the common way. Or with only one cylinder, by admitting a proportionally small quantity of high steam into it from the boiler, Mr. Woolf, found that he could effect a considerable saving in fuel.

In this first improvement of Mr. Woolf, though the saving might be carried a considerable length, it was still necessarily limited by the strength of materials; for in the employment of high steam there must always be some danger of an explosion. Mr. Woolf, however, by a happy thought, has completely obviated every danger of this kind, and can now take the full advantage of the expansive principle without the least danger whatever. This he effects by throwing into common steam the additional temperature necessary for its high expansion, *after the steam is admitted into the working cylinder*, which is heated by means adequate to the end intended to be gained; and the advantage which he thus gains he effectually secures by a most ingenious improvement in the piston. It may be easily conceived that steam of such high rarity as Mr. Woolf employs, could not be made fully effective with the piston in common use; for in proportion to its rarity so must be the facility with which a portion of it would escape, and pass by the side of the piston to the vacuous part of the cylinder: but Mr. Woolf's contrivance seems perfectly adapted to prevent the loss of even the smallest portion of the steam.

Besides

Besides these improvements on the common steam-engine, he has also found means to apply the same principles to the old engine, known by the name of Savary's, in such a way as to render the same a powerful and œconomical engine for a great variety of purposes.

Such is the outline of Mr. Woolf's improvements on this useful engine: but, for the general information of practical engineers, we shall subjoin a more technical description, in Mr. Woolf's own words, extracted from his specification of his patent.

(To be continued.)

SCIENTIFIC NEWS.

Geometry.

Two theorems from the Horologium of Huygens.

HUYGENS has given the two following theorems in his *Horologium Oscillatorium*, which are applicable to all solid bodies: " *The center of oscillation, and that of suspension are always reciprocal to one another. The same body is always isochronal to itself, while it oscillates round parallel axis's taken at equal distances from the center of gravity.* M. Biot has given a remarkable extension to these two theorems.

—extended farther by M. Biot.

All these parallel axis's form the surface of a right angled cylinder of which the axis passes through the center of gravity. But the analytical expression under which M. Biot presents the theorem of Huygens, made him instantly perceive, that an arbitrary inclination might be given to this axis, the radius of the cylinder being suitably changed at the same time; and that thus according to the different degrees of inclination of the axis, an infinity of cylinders might be obtained. The superficies of which cylinders should have the same property as that of the primitive cylinder. Besides this, the axis without changing its inclination may describe a conical surface about its primitive position, which will multiply the number of cylinders already found, as often times as right lines can be conceived to be drawn on the upper surface of the cone.

5 Astronomy.

Astronomy.

M. Pictet has made an obfervation of an occultation of the pleiades by the moon, on the 19th of *November*, 1804, from the Obfervatory of Geneva. M. Pictet on the occultation of the pleiades by the moon.

An account of an occultation of π fcorpion, obferved on the 17th of *July*, 1803, from the fummit of Cafuleta, a mountain in the kingdom of Spain, was found among the papers of the late M. Mechain, which will appear in the 6th Volume of the Memoirs of the French National Inftitute: this is the laft obfervation of this kind made by a man of fcience, whofe premature lofs the Inftitute will long regret. M. Mechain on the occultation of π fcorpion,

A long fucceffion of obfervations was alfo found among his papers, relative to the comet which he had difcovered from Barcelona in 1793, which will alfo appear in the fame publication. and of the comet in 1793.

Geography.

M. Humboldt has read before the *Inftitute Nationale*, *A Memoir on the Longitude of Mexico*, the capital of the kingdom fo called. The longitude of Mexico determined accurately by M. Humboldt.

Geographers difagree with regard to the pofition of this important point. The confiderable difference which M. Humboldt found between his firft obfervation, and the laft which had been formerly made by others before him, engaged him to repeat it as often as he could, and by different methods. The diftances of the moon from the ftars, and feveral eclipfes of Jupiter's moons, always gave the fame refult, which is doubtlefsly preferable to all thofe which have appeared hitherto.

Electricity.

Since the difcovery of electrical conductors by Dr. Franklin, many philofophers have repeated experiments to eftablifh the identity of electrical fire and lightning, by experiments with fuch infolated conductors. Conductor contrived to prevent accidents,

Thefe experiments fucceeded to the wifh of all who tried them; but it was foon perceived, that they were attended with much danger: and fince the death of Profeffor Richman, of Peterfburg, who was ftruck by lightning from his conductor in 1753, few have ventured to repeat the experiment.

M. Beyer

can be insolated or not, at pleasure.

M. Beyer of Paris has formed in his garden an apparatus of this kind, which is very simple, and at the same time perfectly effectual without any danger: It is a conductor which can alternately at pleasure be insolated, or not insolated, and made to act either with a ball, or with a point. The communications between it and the earth are well established, and as the observations can be made at more than an hundred feet from the apparatus, there is not the least danger of any accident.

Aerostation.

Balloon projected by Mr. Robertson, 132 feet diameter, to carry up 50 men,

The celebrated Aeronaut Robertson, who ascended from Petersburg last year, is endeavouring to obtain the necessary assistance at that place for the construction of an air balloon on a very large scale; he proposes that it shall be 132 feet in diameter, which he calculates will carry up 37 ton, and which he supposes, therefore, will easily support 50 people, and all necessary accommodations for them.

and a vessel with sails, &c.

It is to have attached to it a vessel furnished with masts, sails, and every other article requisite for navigating the sea in case of accidents, and provided with a cabin for the aeronauts, properly fitted up, galley for cooking, proper stores for stowing provisions, and several other conveniencies. To render the ascent more safe, it is to take up another smaller balloon within it, and a parachute, which will render the descent perfectly gentle, if the outer balloon bursts.

and an internal balloon and parachute.

From its construction it will be calculated to remain in the air several weeks, in which time many experiments in natural philosophy, and astronomical observations may be made: It is also supposed, that geography may be considerably improved by its means, as the aeronauts will be neither stopped in their course by mountains or forests; and some have even thought, that with the assistance of the trade winds, a voyage round the earth might be made in it between the tropics. Its cost, it is calculated, will be nearly equal to that of a ship of the line.

University of Charkow.

The court of Petersburg published the act of confirmation of the University of Charkow, on the 16th of May, of which the following are the chief particulars.

The

The University is under the care of the Minister of public instruction: It has, however, its own particular administration and jurisdiction; the ordinances, by which its members are governed, are regulated by itself: It has the right of censure both with regard to the books printed by itself, or those brought from abroad. All articles which it may want are allowed to pass the frontiers without examination or tax. Its correspondence is post free, and its paper is not subject to duty. The houses of the professors are free from taxes and all charges. The professors have the rank of the seventh class, and the students that of the twelfth, or that of subaltern officers, receive commission as such, and wear swords. The professors after twenty-five years duty, or in case of incurable sickness, receive their pensions for life, and may even receive them while resident in other countries. On the death of a professor, his widow and children continue to receive his pension, until the widow marries again, or the children attain the age of twenty-one years. The Emperor has granted a yearly revenue to the University of 130,000 rubles.

Forms its own ordinances and regulations.

Various privileges,

Provision for professors, &c.

Botanic Garden, &c. at Copenhagen.

A sum of 4,500 rix dollars, which the government had granted in 1803, for the Botanic Garden at Copenhagen, has been employed, partly in paying the debts of the establishment, and partly in constructing a new hothouse. This garden, which possesses 5,500 plants of different kinds, is open one day in each week for the curious, and every day for botanic students. The directors in their last report, having made some proposals for the improvement and establishment of the garden, the government has granted them an additional sum of 4000 rix dollars, and an annual sum of 200 for repairs; and have besides settled, that the appointments of persons employed in the garden, shall be increased to 720 rix dollars, to commence this year.

has 5500 plants for inspection,

additional grants made to it.

M. Giesecke, a Prussian mineralogist, who has been a considerable time at Copenhagen, is about to be employed by government on a voyage to Greenland, where he is to pass some years in examining that country, with regard to its mineralogy and geology. Hitherto the Moravian religious missionaries have alone been able to resolve to live some years

Proposed mineralogical expedition to Greenland.

years in that country for the conversion of the natives: It will be no little honour to the sciences, if M. Giesecke shall bring himself to make a like sacrifice for their advancement.

Charts printed by moveable types.

The Royal Academy of the Fine Arts, and the Mechanical Arts of Berlin, have received among their members M. F. H. Wagener, who has discovered a new method of printing geographical charts by a species of moveable types, which is found to answer better than engraving, and will undoubtedly be much cheaper.

Prizes given by the French war minister for

Marshal Berthier, war minister of France, at the request of general Marescot, has again established the prizes which was given for the best works on fortification.

a work on subterranean fortification,

Two prizes have been granted to the best *treatises on subterraneous works.* The first was adjudged by the committee of fortification to Major Mouze, the second to Captain Gillot; the committee has adjudged a third treatise deserving of honourable mention, which has for its inscription *artem experientia fecit;* the author of it is not known.

and one on a plan for a fortified barrack.

Another prize on the subject of a *project for a fortified barrack,* has been given to Captain Laurent. The committee have judged the project of Captain Bioschevalier, and that of Lieutenant Colonel Gesbert, to deserve honourable mention. The committee has rejected, for not corresponding with the proposed subject, a project of Captain Mallet, for *a barrack intrenchment;* but have thought it worthy of particular mention, as a work which gives a very advantageous idea of the talents of this officer.

Many of the works which neither received prizes, or particular mention, exhibit ingenious contrivances, and interesting observations. In general these two contests have fully proved the goodness of the Institution, of which the object is to exite emulation in all the corps of the army, to propagate knowledge among them, and to extend the perfection of all the branches of the military art.

Catalogue of Leipsic fair, contained 3647 publications.

THE catalogue of the Leipsic fair, has this year contained two sheets more than usual. The musical publications have been

been added to it. It contains 3,647 articles, furnished by 380 booksellers. The number of romances is 271, of theatrical pieces 81, and music 95, forms 447 articles.

Publication of Arabic antiquities of Spain.

The academy of painting and sculpture at Madrid, are about to publish a compleat collection of the Arabic antiquities of the kingdoms of Grenada and Cordova. In this work will be found not only views and plans of the monuments, and other remarkable matters of these countries, but also an explanation of all the inscriptions, cyphers and hieroglyphics.

Dictionary of the Bunda language, serviceable to those who have business in Africa.

THERE is soon to be published at Lisbon, a Dictionary of the Angola or Bunda language, with the explanation of all the words in Portuguese. There has never before been a dictionary of this language. This will be published for the use of those Portuguese who have business to transact with the establishments which their country possesses on the coast of Africa. No language is spoken there to so great an extent as this.

Mausoleum of Christina, by M. Canova,

THE celebrated sculptor M. Canova, is engaged in erecting at Vienna, the splendid Mausoleum of the Arch-dutchess Christina, an immense composition of eight marble figures, larger than life; the models and execution of which have been long admired at Rome, where they were formed.

and his Theseus and Centaur.

M. Canova before his departure from Rome exhibited a colossal group, representing Theseus combating with a Centaur. This group is to be executed in marble for the city of Milan. The artists and connoisseurs of Rome seem to esteem this work superior to every other which has been executed by this ingenious and indefatigable artist.

Piracy of books punished at Augsburg.

THE Magistracy of Augsburg have had the honour of being the first government of south Germany, which have taken decisive measures against the shameful traffic of book-piracy. It has confiscated the entire edition, consisting of 500 impressions, of the work of Goener, on the political rights

rights of Germany, which was pirated by Kranzfelder, a dealer in such transactions, and has besides compelled Kranzfelder to pay to the legitimate editor, the price of the copies which he had sold.

Russian marine Institution.

THE Russian government have formed at Petersburg, an Institution, whose design is the perfecting of all that belongs to naval armaments, and which is to be called the *Marine Museum*. This institution is not merely to be a school: all the sciences necessary to a naval officer will be there taught and the *Museum* will besides publish a journal which will treat of every thing relative to the marine. It will have also a cabinet of natural history, which will be open to all the pupils. This establishment will be under the direction of the minister of the marine, and its members are to wear the naval uniform.

Russian establishments for education. Numbers of masters and pupils.

ACCORDING to the report of the minister of public instruction, there is at present in Russia 494 institutions for education, directed by 1475 masters, and attended by 33484 scholars. The expence of these establishments costs government annually almost two millions of Rubles. Among these are not reckoned those for the corps of cadets, or for pages, the Academy of Arts, the Schools of Commerce, nor the Institution for Female Education. Those who know the state in which Russian education was at the accession of Alexander, may judge by this detail what he has done towards enlightning his vast empire.

The Russian catholics earnestly concur in seconding his views. At an ecclesiastical assembly, convoked by the bishop of Luik and Shetomir, various measures have been taken in favour of the establishments for education.

Theatre in the Crimea.

IN the town of Odessa in the Crimea, a theatre is building with much activity, according to the plans of M. Thomas de Thomon architect to the Emperor, and a professor of the Peterfburg academy.

This

(This shews that the arts are even extending to this hitherto neglected part of the world, which certainly from its fine climate, and many other advantages, merits every attention of its enlightened and humane master.)

THE third volume of the *Geographical Dictionary of the Russian Empire* has been published by the booksellers, Gavy, Popow, and Luby. Geographical Dictionary of Russia.

AN important work is soon expected to appear at Petersburg, by the scientific M. Delaunay, counsellor of state, relative to Siberia, and the bordering countries.

M. KOTZEBUE in his last tour to Naples relates some particulars which he saw in visiting the Museum of Portici, which will be interesting to the admirers of ancient literature. Ancient manuscripts discovered at Portici are

" Eleven young men are at present employed in unrolling the manuscrips, and two copy them. An Englishman called Haiter, is at the head of the establishment. He relates that his assistants are much more expert and expeditious than they were formerly. He has great hope that he shall have the 600 manuscripts, (which yet remain) decyphered, and has little doubt that he shall discover among them an *Ennius* and a *Menander*, as he flatters himself he has already a *Polybius* in hands.

On the day of the visit, a Greek author, hitherto unknown named Kolotos, was discovered; his work is on philosophy. As the names of the author are always inserted at the end of each manuscript, they can never be known until it is entirely unrolled. Seven latin authors have passed through the hands of Mr. Haiter, but all so much damaged that it was impossible to unroll them, which he the more laments as one of them appeared to be a *Livy*, at least it was an historical work written in his style; all that he can discover of it, is, that it begins with an harangue, in which mention is made of the family of Acilius. They have to this time discovered five authors; *Philodemus*, most of whose writings have been found, and among others, a treatise on the *vices which are nearly allied to virtues. Epicurus, Phædrus, Demetrius Phalereus* and the works of Epicurus, Phædrus,

Demetrius, and a Kolotos. and his *Kolotos* above-mentioned. Mr. Haiter regrets that he has hitherto only met with works on philosophy, although among these many historical ideas, hitherto unknown, occur here and there: as happened in a dissertation on anger, in which is cited the example of Cadmus punished by Bacchus for having given himself up to this passion, a circumstance hitherto unknown.

IT has for some time been an object of deliberation with me, to ascertain by what means I might most effectually remedy an inconvenience which has arisen from the distinguished patronage this Journal has been honoured with: The great extent and value of original communications cannot but be duly estimated by the public; though at the same time it has necessarily followed, that various articles of news and other subjects in the foreign Journals, have in many instances been postponed, and in some rejected. To retain all the peculiar advantages of this work, and to afford ample space for occasional and foreign articles of value, the obvious means have appeared, that according to the practice of several other respectable works, each volume should be concluded by a Supplementary Number; containing six sheets, or 96 pages of printed matter, and two plates. And, as many of the former plates, like those in the present number, have contained mathematical figures or outline delineations, capable of being advantageously condensed, it is purposed in all the future numbers to give two very full plates, and sixteen extra pages of matter, instead of the four plates hitherto given. By this arrangement every volume will in future contain 30 sheets or 480 pages of matter, and 10 full plates; instead of 20 sheets or 320 pages, with 16 plates less fully occupied. This addition of new matter to the amount of full one half more, will admit the insertion of many interesting articles which want of room must also have excluded.

*** *The plate of Rye-Harbour could not be finished in time on account of the sudden illness of the Engraver. It will be given gratis in the Supplement, which will be published Jan. 1. next, at the same time as No. 50.*

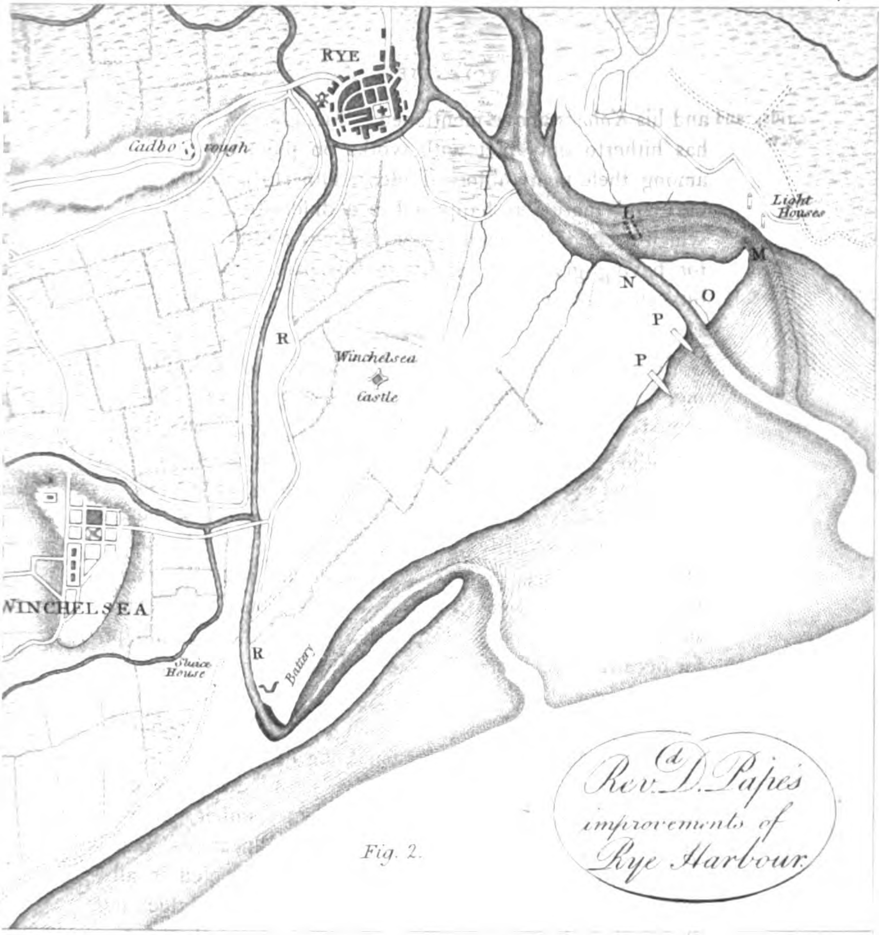


Fig. 2.

Cross Section of M^r. Pape's Dam.

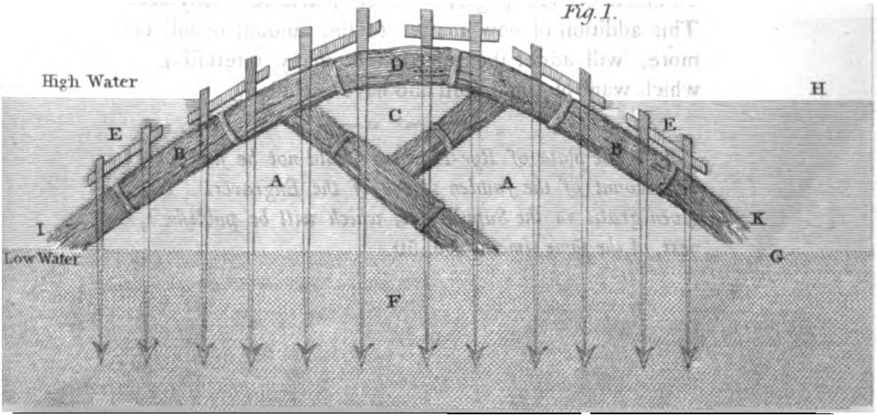


Fig. 1.

A

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AND

THE ARTS.

SUPPLEMENT TO VOL. XII.

ARTICLE I.

A Description of an Air Pump upon a new Construction. By ELIZUR WRIGHT. *Communicated by* BENJ. SILLIMAN, *Esq. Professor of Natural Philosophy, &c. in Yale College, Newhaven, America.*

UPON reading the improvements made in the air pump by Smeaton, Haas, Prince, Russel, and Cuthbertson, it occurred to me that the end which they aimed at might in some measure be attained upon a principle that is different from either of those by which their pumps have been constructed It is well known that in a common air pump the valve at the bottom of the barrel depends upon the air in the receiver to open it. When the air in the receiver is rarified to a certain degree, its spring becomes too weak to overcome even the small resistance which will arise from the weight of the valve, its cohesion to the plate occasioned by the oil, and its being stretched tight over the hole. Here the progress of exhaustion will stop. And this would hold true, could it be possible to produce a perfect vacuum in the barrel. But as the same obstructions belong to the piston valve, together with the additional one arising from the pressure of the external air upon it, and because the piston can-

The general imperfections of the air pump explained.

not be fo accurately fitted to the lower valve as, when put down upon it, to leave no vacuity between them; a portion of air will neceffarily be retained in the barrel, which by its preffure ftill further prevents the opening of the lower valve, and caufes the operator to come to the limit of rarefaction much fooner than he would upon the fuppofition that a perfect vacuum were made in the barrel. Several very ingenious contrivances have been invented to remove this imperfection, among which thofe of Cuthbertfon and Prince are among the lateft, and cannot fail of giving the reader a very high idea of their fagacity and talents for invention. The method ufed by the Rev. Mr. Prince, of removing the lower valve by opening the bottom of the barrel into a ciftern which has a communication with the receiver, firft gave the hint that it might be poffible in fome fimilar way to difpenfe with both the valves, and by this means carry the air pump to a greater degree of perfection. In purfuing this fubject I found that all this might be effected, and in a way that admitted of much fimplicity of conftruction.

Improvements of Cuthbertfon and Prince.

The air-pump of Prince improved.

Defcription of a new air pump.

The principle upon which this pump operates may be feen in the following defcription of it. F, (*Plate* XIV. *Fig.* 1.) is the pump plate. O C is the barrel lying in a horizontal pofition underneath the pump plate, and nearly in contact with it. A and B are two ducts leading from the pump plate into the barrel. The pifton P is without a valve, being folid and accurately fitted to the barrel. The pifton rod M is cylindrical and moves air tight in the leathern collar O. There is another pifton, N, made like the former, but fhorter, and acted upon by the fpring S, which is thence termed the fpring pifton. The ends of thefe piftons are very carefully fitted to each other, fo that when they are brought into contact they will form one uniform cylinder without any vacuity betwixt them. H is the winch, with a pinion and rack by which it is worked. The pump is fupported by a pedeftal upon which it is firmly fixed.

A folid pifton works in a barrel. Its rod paffes through a collar of leather. Two holes in the barrel ferve, one to admit air from the receiver and the other to difcharge it.

The manner in which it operates is this: Suppofe the receiver placed over the duct A, leaving the duct B open to the external air, alfo the fpring pifton in the fituation N, excluding the external air from the barrel, as reprefented in the figure and the pifton P in contact with it. The pifton P, by moving towards the duct A, forms a vacuum in the barrel. When it paffes by the duct A, it opens a communication between the receiver

Mr. Gough's Propositions respecting a Division of the Arch of a Circle.

Fig. 2.

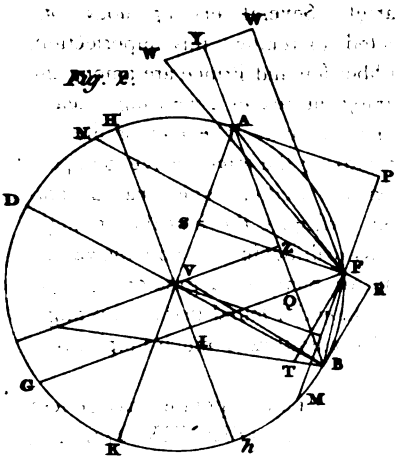


Fig. 3.

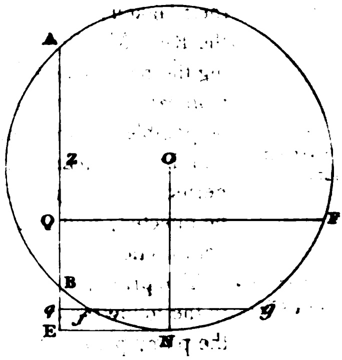


Fig. 1.

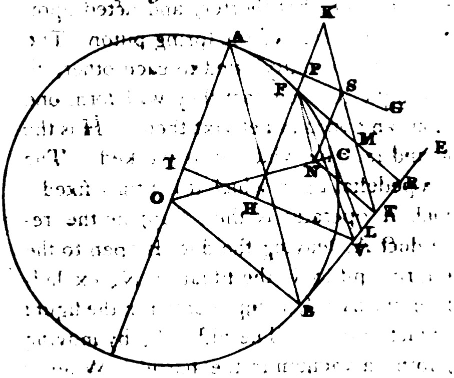
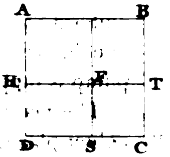


Fig. 4.



Mutlow Sc. Russell Ct.

Dr. Herschell's investigation of the power of telescopes to distinguish the figures of very minute objects.

Fig. 1.

Fig. 2.

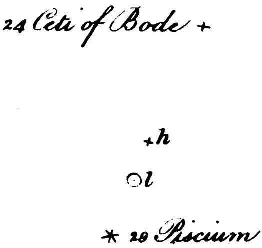


Fig. 4.

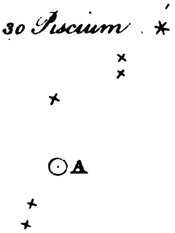


Fig. 3.

Mutlow Sc. Russell Ct.

[illegible] into [illegible]
barrel and fill [illegible] The [illegible] towards the
[illegible] B, and drives before it [illegible] in the barrel,
[illegible] spring [illegible] N, [illegible] stopped by
[illegible] D [illegible] two pi-
[illegible]

receiver and barrel, and the air by its elastic force rushes into the barrel and fills it. The piston now returns towards the duct B, and drives before it the air contained in the barrel, together with the spring piston N, until they are stopped by the shoulder D at the instant in which the ends of the two pistons come against the middle of the duct B. By forcing the air out at the duct B, the pistons come into contact, and form one uniform cylinder, that prevents any communication of the barrel with the external air. The piston P is now drawn back toward the duct A, and the spring piston N, by the action of its spring, follows in close contact with it, until it is stopped by its shoulder C meeting with the end of the barrel, after having passed the duct B, and having continued to intercept the communication between the barrel and the external air. This is the situation with which the description began; and, repeating the operation, when the piston P is drawn back beyond the duct A, the air from the receiver rushes into the barrel; and, when it moves forward to the duct B this air is expelled.

The latter is closed by a spring piston.

Having exhibited a general description of this pump, with the manner of its working, a more particular illustration of some of its parts will be given.

When the pump is intended to exhaust, the receiver must be placed over the duct A, leaving the duct B open to the external air; but when it is designed to condense, nothing more is necessary than to shift the situation of the receiver on the plate, placing it over the duct B, and leaving the duct A open to the external air.

This pump exhausts or condenses at pleasure.

The duct B is continued around the spring piston by means of a circular channel cut into the inside of the barrel, in order that the air might escape from all sides when the pistons come into contact.

It may be observed that all the back space in the barrel between the collar O and the piston P makes a part of the capacity of the receiver; or, to speak more accurately, the space O A between the collar O and the duct A: the space A P between the duct and piston, while it moves from A toward B, being only a temporary dilation of the capacity, and the space A P while it moves from A towards O a temporary contraction of it.

Observations.

For the purpose of preventing a fluctuation of the air in the receiver, which would be caused by this expansion and contraction,

Advantage of constructing it with two barrels.

traction, and might be detrimental in some experiments, the diameter of the duct A is made very small, and another barrel, having similar pistons and ducts, is added, with its rack placed above the pinion wheel, while the other is placed below it. The advantage of a pump of this kind being constructed with two barrels arises from the contrary motions of their pistons; for while one augments the capacity of the receiver by moving forward, the other equally diminishes it by moving backward. An equilibrium is thus maintained that prevents any oscillatory motion in the mercury of the gage, which might arise from the operation of a single barrel.

The resistance from the spring piston.

The additional resistance to be overcome in working this pump, above what is to be met with in other pumps, happens only at the small interval while the spring piston is passing from its natural situation to the duct B. This need not be more than about four times greater than that which is requisite to overcome the friction of the piston P, and will be easily provided or by increasing the proportion between the diameter of the pinion wheel and the sweep of the handle.

ELIZUR WRIGHT, *C. A. S.*

Canaan (Connecticut in America,)
March 12, 1805.

II.

Concerning the State in which the true Sap of Trees is deposited during Winter. *By* THOMAS ANDREW KNIGHT, *Esq.**

(Concluded from Page 240)

Bulbous and tuberous roots contain the matter that forms leaves,

WE have much more decisive evidence that bulbous and tuberous rooted plants contain the matter within themselves which subsequently composes their leaves; for we see them vegetate even in dry rooms, on the approach of spring; and many bulbous rooted plants produce their leaves and flowers with nearly the same vigour by the application of water only, as they do when growing in the best mould. But the water in this case, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by dissolving the

* Phil. Trans. of 1805, p. 97.

matter

matter prepared and deposited in the preceding year; and hence the root becomes exhausted and spoiled: and Hassenfratz found that the leaves and flowers and roots of such plants afforded no more carbon than he had proved to exist in bulbous roots of the same weight, whose leaves and flowers had never expanded.

As the leaves and flowers of the hyacinth, in the preceding case, derived their matter from the bulb, it appears extremely probable that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many species precede their leaves: and, as the roots of plants become weakened and apparently exhausted, when they have afforded nutriment to a crop of seed, we may suspect that a tree, which has borne much fruit in one season, becomes in a similar way exhausted, and incapable of affording proper nutriment to a crop in the succeeding year. And I am much inclined to believe that were the wood of a tree in this state accurately weighed, it would be found specifically lighter than that of a similar tree, which had not afforded nutriment to fruit or blossoms, in the preceding year, or years.

—and it is highly probable that trees contain the nutriment of their fruits; &c.

If it be admitted that the substance which enters into the composition of the first leaves in the spring is derived from matter which has undergone some previous preparation within the plant, (and I am at a loss to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at least,) it must also be admitted that the leaves which are generated in the summer derive their substance from a similar source; and this cannot be conceded without a direct admission of the existence of vegetable circulation, which is denied by so many eminent naturalists. I have not, however, found in their writings a single fact to disprove its existence, nor any great weight in their arguments, except those drawn from two important errors in the admirable works of Hales and Du Hamel, which I have noticed in a former memoir. I shall therefore proceed to point out the channels, through which I conceive the circulating fluids to pass.

The preparation of this nutriment in the tree implies that the juices circulate.

When a seed is deposited in the ground, or otherwise exposed to a proper degree of heat and moisture, and exposure to air, water is absorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every subsequent stage of the growth of the root, it increases in length by the addition of new parts to its apex, or point, and not by any general dis-

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

tension

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

tension of its vessels and fibres; and the experiments of Bonnet and Du Hamel leave little grounds of doubt, but that the new matter which is added to the point of the root descends from the cotyledons. The first motion therefore of the fluids in plants is downwards, towards the point of the root; and the vessels which appear to carry them, are of the same kind with those which are subsequently found in the bark, where I have, on a former occasion, endeavoured to prove that they execute the same office.

In the last spring I examined almost every day the progressive changes which take place in the radicle emitted by the horse chesnut: I found it, at its first existence, and until it was some weeks old, to be incapable of absorbing coloured infusions, when its point was taken off, and I was totally unable to discover any alburnous tubes, through which the sap absorbed from the ground, in the subsequent growth of the tree, ascends: but when the roots were considerably elongated, alburnous tubes formed; and as soon as they had acquired some degree of firmness in their consistence, they appeared to enter on their office of carrying up the aqueous sap, and the leaves of the plumula then, and not sooner, expanded.

The leaf contains at least three kinds of tubes: the first is what, in a former Paper, I have called the central vessel, through which the aqueous sap appears to be carried, and through which coloured infusions readily pass, from the alburnous tubes into the leaf-stalk. These vessels are always accompanied by spiral tubes, which do not appear to carry any liquid: but there is another vessel which appears to take its origin from the leaf, and which descends down the internal bark, and contains the true or prepared sap. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon; but being exposed to the air, and without the same means to acquire, or the substance to retain moisture, it is fed by the alburnous tubes and central vessels. The true sap now appears to be discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark, and to be employed in the formation of new alburnous tubes between the base of the leaf and the root. From these alburnous tubes spring other central vessels and spiral tubes, which enter into and possibly give existence to, other leaves; and thus by a repetition of the same [illegible] process

process the young tree or annual shoot continues to acquire new parts, which apparently are formed from the ascending aqueous sap.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

But it has been proved by Du Hamel that a fluid, similar to that which is found in the true sap vessels of the bark, exists also in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whose true sap is white, or coloured. The vessels, which contain this fluid in the alburnum, are in contact with those which carry up the aqueous sap; and it does not appear probable that, in a body so porous as wood, fluids so near each other should remain wholly unmixed. I must therefore conclude that when the true sap has been delivered from the cotyledon or leaf into the returning, or true sap vessels of the bark, one portion of it secretes through the external cellular, or more probably glandular substance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it secretes through the internal glandular substance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and subsequently mingles with the ascending aqueous sap; which thus becomes capable of affording the matter necessary to form new buds and leaves.

It has been proved in the preceding experiments on the ascending sap of the sycamore and birch, that that fluid does not approach the buds and unfolding leaves in the spring, in the state in which it is absorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, consists of a mixture of the true sap or blood of the plant with matter more recently absorbed, and less perfectly assimilated.

It appears probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap; for this fluid in the sycamore has been proved to become more sensibly sweet in its progress from the roots in the spring, and the liquid, which flows from the wounded bark of the same tree is also sweet; but I have never been able to detect the slightest degree of sweetness in decoctions of the sycamore wood in winter. I am therefore inclined to believe that the saccharine matter existing in the ascending sap is not immediately, or wholly, derived from the fluid which had circulated through

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

through the leaf in the preceding year; but that it is generated by a procefs fimilar to that of the germination of feeds, and that the fame procefs is always going forward during the fpring and fummer, as long as the tree continues to generate new organs. But towards the conclufion of the fummer I conceive that the true fap fimply accumulates in the alburnum, and thus adds to the fpecific gravity of winter-felled wood, and increafes the quantity of its extractive matter.

I have fome reafons to believe that the true fap defcends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the fpring, and fuch trees be fuffered to grow till the following winter, the alburnum acquires a great degree of hardnefs and durability. If fubfequent experiments prove that the true fap defcends through the alburnum, it will be eafy to point out the caufe why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why fome portion of alburnous matter is in all trees* generated below incifions through the bark.

It was my intention this year to have troubled you with fome obfervations on the reproduction of the buds and roots of trees; but as the fubject of the Paper, which I have now the honour to addrefs to you, appeared to be of more importance, I have deferred thofe obfervations to a future opportunity; and I fhall at prefent only obferve, that I conceive myfelf to be in poffeffion of facts to prove that both buds and roots originate from the alburnous fubftance of plants, and not, as is, I believe, generally fuppofed, from the bark.

I am, &c.

T. ANDREW KNIGHT.

Elton, Dec. 4, 1804.

* I have in a former paper ftated that the perpendicular fhoots of the vine form an exception. I fpoke on the authority of numerous experiments; but they had been made late in the fummer; and on repeating the fame experiments at an earlier period, I found the refult in conformity with my experiments on other trees.

Singular

III.

Singular Method of forming Walls and Roofs of Rural Buildings, in Indostan, communicated by M. LEGOUX DE FLAIX, *Officer of Engineers* *.

THE method which the Indians have used for many years, of forming their rural buildings, unites solidity, convenience, and wholesomeness to economy, and facility of execution. **Advantages of this method of building.**

Houses constructed in this manner have also the advantage of being absolutely safe from conflagration, and of resisting even the most violent inundations. **Resists fire and inundations.**

In a country where stone is scarce, the rich build their houses with bricks, which in many respects are preferable to stone; but poor people, such as those employed in agriculture, cannot go to that expence, even in India, where labour and materials are so cheap.

The habitations of villagers in most parts of the globe are built with earth walls, in India they are likewise covered with terraces of earth, and it is evident, that buildings formed with both walls and roofs of earth must necessarily be free from danger of fire.

To prove that buildings of this construction are equally safe from inundations, it is sufficient to state, that on the banks of the Ganges and Indus, (rivers of vast magnitude both in their extent and their course, and whose great bodies of water cause the most destructive effects in their floods,) these houses stand uninjured, though sometimes isolated in the midst of immense inundations for fifteen or twenty days. It is extremely probable, that houses built of stone or brick would not stand this great force of water equally well.

To form houses in this manner, the foundations of the outside and partition walls are dug up, which are sometimes from five to seven feet deep, and always proportioned to the heighth intended to be given to the walls. The excavated earth is exposed till it becomes perfectly dry; if it is of a fat or argillaceous nature, it is carried to a place, prepared for the purpose, where it may be pounded into a dust, and properly prepared for use; when in this state, it is mixed with a **The earth dug from the foundations is pounded fine, and mixed with coarse sand or fine gravel,**

* Sonninus's Journal, II. 394.

third or an half of coarse sand, or small gravel passed through a sieve to clear it from pebbles. The fat earth is mixed with the sand and gravel, and worked up well with it, so that the mass may be of an uniform consistence. It is then moistened with water five or six hours before it is wanted, and in the quantity necessary for a single day's work alone.

then moistened with water.

The mixture thus prepared is carried to the place of building, when the foundations are perfectly dry, and the walls are then built equally in every part at the same time, on a perfect level, in courses, and brought up perpendicularly; each course of earth is from eight to ten inches in depth, and the whole breadth of the wall, which is seldom less than two feet thick, and never exceeds four; which dimensions are always regulated by the intended height of the building, and the force of the floods, if it is near the river. When the walls are three feet and a half, or four feet thick, only one course is raised in a day; but when they are from two feet and half to three feet thick, two courses are railed, and if they are but two feet thick, three courses are sometimes raised in that space of time. This depends on the quickness of the desiccation of the walls, which speedily takes place there, where the dryness of the air is extreme: this would not perhaps happen in our moist climates; if this method of building should be tried here, it would probably be necessary to leave them longer to dry, in order to obtain the requisite tenacity.

The walls are raised all together, from two to four feet thick. In one two or three courses in a day according to the thickness.

When the walls are built to the height for the roof, the proper openings are made for the beams and joists. It is almost needless to add, that the apertures necessary for the doors and windows are made while the walls are building.

Spaces left for beams, doors, and windows.

On the twelfth or fifteenth day, or when the walls are sufficiently dry, or to the same degree to which tiles are dried, the walls are surrounded externally and internally with a sort of open work case, made of spars of bambou, or of some other hard and dry wood. In Indostan, where this method of building is general, the workman have bars of iron, which they hire out, that serve to sustain the coffer work mentioned, and are placed at every three or four yards; the coffer work is raised at two or three feet distance from the surface of the walls, according to their thickness, and the space between is filled up with firewood, turfs, and cakes made of cow and sheep dung worked together and dried in the sun.

The walls when dry are enclosed in open work cases of bambou, at two or three feet distance, and the interval filled with fuel.

This

This pile of combustibles is arranged in several stages, of three, four, or five feet thick, separated from each other by layers of earth or half dried turf of from eight to ten inches depth; the upper stages are first set on fire, so that the wall is baked through its whole extent from top to bottom. The charge of the combustibles for each of the stages is so managed, that the lowest is the greatest, and is diminished for each as it is nearer the top of the wall; as the pile burns down the fire of the lower stages still acts on the upper part of the walls, which permits the upper stages to be of less thickness. The fire bakes the walls to a thickness of from six to ten inches, as tiles are baked in a kiln. And thus walls are built in a single piece, and of the greatest solidity, which have the more strength, as there are no junctures in them. Wherefore they ought to prevent the greatest possible resistance to the action of the atmosphere, the attacks of floods, and the fall of rain, which descends in torrents in most countries of Indostan during the rainy season.

Arranged in stages separated by layers of earth.

The upper stages first kindled.

Walls thus baked to the thickness of from six to ten inches.

Experience has constantly proved, that the houses built in this manner not only last much longer than those built of bricks, but that they also resist better the attacks of the periodical inundations, and those of the annual rains to which they are exposed in this climate.

These houses last longer than those of brick, and resist floods and rain better.

The method in which the terrace roofs (which are called in India *argamace*) are formed for these houses, is the following:

Their roofs are made of clay in three layers.

Immediately after the baking and cooling of the walls, the ashes and the bars which sustained the coffer work are removed. The beams and joists are placed, and covered either with very thin boards, or else with small green branches; and upon this support the different layers of the terrace roof are placed. The first layer is simply clay, with an equal quantity of *ole*, a species of marl in powder, which is pounded in troughs, such as are used for preparing mortar. This first layer is four or five inches thick, and it is then levelled, and is moistened from time to time, in order to beat it firm with small bats. As soon as this is dry, the second layer is laid on, which consists of potters clay worked up in the same manner as if prepared for making pottery; this layer is only two or three inches thick at most. It is levelled according to the slope of the terrace, which is given it in placing the beams and

First layer common clay and *ole*, a kind of marl, four or five inches thick.

Second layer potters clay, two or three inches thick.

and joists, and it is consolidated by light blows of wooden trowels, until it is perfectly dry. When the clay forms cracks in drying they are closed by other clay prepared for filling up these chinks to the bottoms.

Third layer clay with one fourth brick dust and one fourth fine sand 6 or 8 inches thick.

The second layer when perfectly dry, and free from cracks, is covered with a third layer; which is composed of pulverized clay mixed with a fourth of brick dust, passed through a close sieve, and with a fourth of fine sand. This mixture is worked up in a trough like mortar; it is used as soon as prepared, and is then spread out equally over the whole terrace six or eight inches thick; this layer is consolidated in the same manner as the others, and this labour is continued till it is perfectly dry; and then the *argamace* is finished. This terrace is strong, and has such tenacity that the most violent rains cannot penetrate it.

Houses thus built cost 6 franks for the cubic fathom.

A building of this species costs in India but six francs (five shillings) for the cubic fathom and is entirely performed by masons. In France it would cost three times as much (and something more in England) on account of the greater expence of labour and fuel.

May be made with many stories.

Houses may be built in this manner of any height required, and of as many stories as are thought fit; I have seen some that had but one ground floor; but I have also seen others that were elevated two stories above the ground floor. One of this last sort, situated on the banks of the Gemna in the province of Alabad, was built above 430 years, and the walls, and the whole of the building looked as fresh as if they were new.

An house of this sort 430 years built seemed quite fresh.

IV.

Account of some new Improvements on Steam-Engines. By Mr. Arthur Woolf.

(Concluded from page 296.)

Mr. Woolf's improvements in steam-engines.

"I have found out and invented a contrivance, by which the temperature of the steam vessel or working cylinder of a steam-engine, or of the steam vessels or cylinders where more than one are used, may be raised to any required temperature, without admitting steam from the boiler into any surrounding receptacle, whether known by the name of a steam case, or by any other denomination. That is to say, instead of admitting steam

steam of a high temperature into such receptacle or steam case, which is always attended with a risk of explosion proportioned to the elasticity of the steam employed, I put into the said surrounding receptacle, or case, oil or the fat of animals, or wax or other substances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour: or I put into the said case or cases mercury or mixtures of metals, as of tin, bismuth, and lead, capable of being kept in a state of fusion in a lower temperature than that intended to be employed in working the steam-engine; and I so form the surrounding case or cases as to make it or them admit the aforesaid oil, or other substance employed, to come into contact not only with the sides of the steam vessel or vessels, or working cylinder or cylinders, but also with the bottom and top of the same, so that the whole may be as much as possible maintained in one uniform temperature; and this temperature I keep up by a fire immediately under or round the case or cases that contains the aforesaid oil or other substance, or by connecting the said case or cases with a separate vessel or vessels, kept at a proper temperature, filled with the oil or other substance made use of as aforesaid. In some circumstances, or whenever the same may be convenient or desirable, I employ the fluid metals, or mixtures of metals, and oil or other of the substances before enumerated, at one and the same time in the same engine; that is to say, in the part of the case or vessel exposed to the greatest action of the fire, I sometimes have the aforesaid metals or mixtures of metals, and in the parts less exposed to the action of the fire, I put oil, or other substances capable of bearing the requisite heat without being converted into vapour.

Mr. Woolf's improvements in steam-engines.

"By this arrangement, and method of applying the surrounding heat, I not only obviate the necessity of employing steam of a great expansive force round the steam vessel or vessels, or the working cylinder or cylinders, as already mentioned, to maintain them at the temperature required, but I am enabled to obtain from steam of a comparatively low temperature, or even from water itself, admitted into the steam vessel or vessels, all the effects that can be obtained from steam of a high temperature, without any of the

Mr. Woolf's improvements in steam-engines.

the risk with which the production of the latter is accompanied, not only to the boiler and other parts of the machinery, but even to the lives of the workmen; for such low steam, or even water, (but in every case steam is preferable,) being admitted into a steam vessel or vessels, or working cylinder or cylinders, kept at the requisite higher temperature by the forementioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine which cannot otherwise be obtained but at a greater expense of fuel, or with the risk of an explosion. By this means I can make use of steam expanded in any required ratio, or of any given temperature, without the necessity of ever having the steam of any greater elasticity than equal to the pressure of the common atmosphere.

"Another improvement which I make use of in steam-engines consists in a method of preventing, as much as possible, the passage of any of the steam from that side of the piston which is acted upon by the said steam to the other side which is open to the condenser; and this I effect, in those steam-engines known by the name of double engines, by employing upon or above the piston mercury or fluid metal, or metals in an altitude equal to the pressure of the steam. The efficacy of this arrangement will appear obvious, from attending to what must take place in working such a piston. When the piston is ascending, that is, when the steam is admitted below the piston, the space on its other side being open to the condenser, the steam endeavouring to pass up by the side of the piston is met and effectually prevented by the column of metal equal or superior to it in pressure; and during the down stroke no steam can possibly pass without first forcing all the metal through. In working what is called a single engine a less considerable altitude of metal is required, because the steam always acts on the upper side of the piston. For single engines, oil or wax, or fat of animals, or similar substances, in sufficient quantity, will answer the purpose, if another improvement, which constitutes part of my said invention, be applied to the engine, namely to take care that in either the double or single engine so to be worked, the outlet that conveys the steam to the condenser shall be so perfected, and of such a size, that the steam may pass without

forcing

forcing before it or carrying with it any of the metal or other substance employed, that may have passed by the piston; taking care at the same to provide another exit for the metal or other substance collected at the bottom of the steam vessel or working cylinder to convey the same into a reservoir kept at a proper heat, whence it is to be conveyed to the upper side of the piston by a small pump worked by the engine or by any other contrivance. In order that the fluid metal or metals used with the piston may not be oxidated, I always keep some oil or other fluid substance on its surface, to prevent its coming in contact with the atmosphere; and to prevent the necessity of employing a large quantity of fluid metal, I generally make my piston of the depth of the column required, but of a diameter a little less than the steam vessel or working cylinder, excepting where the packing or other fitting is necessary to be applied; so that, in fact, the column of fluid metal forms only a thin body round the piston. In some cases I make a hollow metallic piston, and apply an altitude of fluid metal in the inside of the working cylinder.

Mr. Woolf's improvements in steam-engines.

"It may be necessary, however, to state, that in applying my improved method of keeping the steam vessels of steam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a separate condenser has been introduced, I sometimes employ oil (or any other substance lighter than water, and capable of being kept fluid in the temperature employed, without being converted into vapour,) in the upper part of the tube or pipe attached to the steam vessel; by which means steam of any temperature may be used without being exposed to the risk of partial condensation by the admission of any colder body into the steam vessel; for the oil, or other substance employed for this purpose, soon acquires the requisite temperature; and to prevent unnecessary escape of heat, I construct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the steam vessel which may not be heated exteriorly. And further, (as is already the practice in some engines, and therefore not exclusively claimed by me,) I cause the water raised by the engine to pass off through another ascending tube than the one attached to the steam vessel, but connected with it at some part lower than the oil or other substance employed in it is ever suffered to descend to in the working

working of the engine. The improvement which I have just mentioned, of introducing oil into the pipe attached to the steam vessel of such engines, may also be introduced without applying heat externally to the steam vessel; but in this case part of the effect which would otherwise be gained is lost."

V.

On the Magnesian Earth of Baudissero. By M. GIOBERT.

(Concluded from Page 284.)

The earth of Baudissero analysed.

TO ascertain the proportions of these constituent parts of the magnesian earth, we lixiviated a given weight of it, and precipitated the sulphuric acid from one part of it by acetite of barytes, and the lime from the other part by oxalate of ammonia.

It contains, besides magnesia, sulphate of lime,

The weight of the oxalate of lime, and that of the sulphate of barytes, obtained from it, shewed us that it contained 1,60 of the sulphate of lime. The experiments before recited determine the proportion of the silex contained.

—with silex,

—and carbonic acid,

To prove that of the carbonic acid, we both calcined a given weight of the earth in crucibles, from which syphons passed into bottles containing lime water; in order that the carbonic acid gas furnished by the earth might be precipitated, and also dissolved considerable quantities of it in acids by the action of heat, and received the gas produced in bottles filled in like manner with lime water; the first method produced constantly the most. The carbonate of lime formed in these different experiments apprized us that 100 parts of the earth contained from 8 to 12 of carbonic acid, and sometimes a little less in the stony species.

—and water.

If this weight of the carbonic acid be deducted from the loss of weight which this earth suffers by the calcination in the fire, which was mentioned before, we shall then have the quantity of water which the earth contains. In collecting the results of the different experiments, it appears that the earth of Baudissero is composed of

Magnesia

Magnesia	68	Proportions tabulated.
Carbonic acid	12	
Silex	15,60	
Sulphate of lime	1,60	
Water	3	
	100,20	

It is from these results that I denominate this earth native magnesia. It is doubtless found mixed with a little silex; but if the title of native alumen is given to the alumenous earth of Halle in Saxony, which contains 24 parts of the sulphate of lime; if the name of native magnesia is given to that of Moravia, announced by Mitchael, of which 100 parts contain 50 of carbonic acid; it appears to me that the earth which I describe has a much better title to the name which I have given it.

It may be called with propriety native magnesia.

The earth of Baudissero affords a subject for interesting observations in the investigation of its origin. Many facts lead me to believe that this earth and the Corneen stone or Cacholong, described and analysed by my colleague Bonvoisin, are both of the same nature. It appears to me that Cacholong at a certain point of its decomposition forms what Bonvoisin calls the hydrophane of Piedmont, and that in its complete decomposition it forms the magnesian earth of which I here give the analysis. Bonvoisin has declared himself of an opinion precisely contrary to this; for he has supposed that this earth, far from being the product of the decomposition of Cacholong, is the element of its formation. Our colleague Gioanetti is of the same opinion. In these two hypotheses, the change of one earth into another is manifest, that is to say, the change of silex and alumen into magnesia in my method of considering the matter; (for it is principally of these two earths that Cacholong and Hydrophane are composed, from the analysis of Bonvoisin;) and the change of magnesia into alumen and silex according to the hypothesis of Bonvoisin and Gioanetti. As this subject appears to me to be very interesting, I intend to make a comparative analysis of these stones at the different degrees of their decomposition or entering into the state of agate (agatisation) which shall be the subject of another memoir.

Supposed to be produced by the decomposition of the Corneen stone or Cacholong.

And therefore that either alumen and silex is changed into magnesia, —or the converse.

There remains yet for me to examine the economical uses for which this earth may be employed.

The experiment which I related in the beginning of this memoir, of the decomposition of the sulphate of iron by this earth, which produced an excellent sulphate of magnesia, indicates one of the methods in which it might be used to advantage.

Sulphate of magnesia may be manufactured from it to advantage by the process with sulphate of iron, described before, more pure than that of commerce in general.

Twenty-five pounds of sulphate of iron cost only three francs with us, while the price of the same weight of sulphate of magnesia is eight francs, from this it follows that this process may be followed to advantage. To this may be added that the sulphate of magnesia of commerce, being impure, and mixed with much sulphate of soda, cannot be compared to that which may be procured in this manner, which equals the best salt from * * canal; so that in this comparison the more pure sulphate of magnesia thus obtained, may be valued at ten francs at least, and in reality is worth more.

This however is not the best method to pursue, when the operator has it in his power to follow the others, which I am going to recite.

The following experiments make known two processes much more economical.

In the first experiment I took two pounds of the earth of Baudissero, reduced to a coarse powder, with the same quantity of the sulphuret of iron of Brozo reduced to powder in like manner, I mixed them together carefully, and treated one half in a crucible on the fire, and the other half in an iron capsule.

Or by pounding these and heating to redness in crucibles:

In both the mixture heated to redness emitted sparks, especially on being stirred. It seemed to become reduced to a very fine powder; a sort of boiling took place, produced doubtlessly by the disengagement of carbonic acid, and here and there appeared flames of sulphur, which burned without exhibiting any sign of the production of a sulphuret. The sulphurous odour was not however very troublesome, from whence it appeared that the magnesia absorbed with readiness the sulphuric and sulphurous acids in proportion as they were formed by combustion. The mixture became of a blackish grey, or more properly a black; but which appeared grey from the white particles which still remained mixed with it.

After

After being left three hours to cool, it was moistened with water and put away till next day, a part of it was then lixiviated; the solution being made clear and treated with ammonia, gave an abundant and very white precipitate. This circumstance indicating that much of the magnesia was combined with sulphuric acid in the operation, all the remainder was lixiviated. The very clear lixivium, evaporated properly, produced at the first crystalization a pound of sulphate of magnesia in beautiful crystals. The remaining liquor gave on successive evaporations a pound and half more of the same salt in fine crystals, very dry and very white. The liquor produced crystals to the last drop, and the mother-water never became foul.

Leaving to digest some time, moistened with cold water, Lixiviating, and crystalizing, fine crystals in abundance are had.

The mixture which remained after lixiviation was roasted a second time, and again produced sulphate of magnesia: It was then thrown away, although apparently it would have yielded more sulphate of magnesia after another torrification.

The mother-water, evaporated successively gives crystals to the last drop. The residuum roasted again and re-lixiviated gives more crystals.

In another experiment, pure sulphur was used instead of the pyrites; it was easy to foresee that the result would be the same; it was however desirable to prove it; and the result was perfectly satisfactory.

Pure sulphur used in place of the sulphate of iron answers equally well.

The use then which may be made of this earth, consists in forming with it sulphate of magnesia. The means by which this may be done are perhaps the most simple possible. It is sufficient to reduce to powder the earth and the sulphur, or the sulphuret of iron, where it can be easily procured, as may be done at Baudissero. These substances should be mixed in almost equal parts; for it is useful to proceed with an excess of the earth, and the more so, as its cost is almost nothing: The mixture should be torrified in an oven or kiln, heated to the degree at which sulphur inflames, and when there appear no more jets of sulphurous flames, the kiln is to be left to cool. The matter being then drawn out should be moistened with water in cisterns, and left for some days, only taking care to stir it in that time.

Process for sulphate of magnesia in the large way with magnesian earth and pyrites, similar to the foregoing.

The part of the sulphur which in burning had only passed to the state of sulphurous acid, oxigenates gradually, or the salt, which at first was but a sulphite changes to a sulphate. The matter is then to be lixiviated, in the same manner that is used for nitrous earths, the liquor sufficiently evaporated, and left to crystalize by cooling.

Another process in the large way, where pyrites are burned. The kiln may be covered with a heap of magnesian earth.

Another method may be followed in places where sulphurets are worked; or where, as at Brozo, there is a manufacture of sulphate of iron. The kiln, where the pyrites are burned, may be covered with an heap of the magnesian earth; the sulphuric acid, which is disengaged will be absorbed by the magnesia; and to the advantage of putting an end to the complaints of the owners of property near the manufacture, will be added that of *sulphating* the magnesia, from which the salt may afterwards be procured by lixiviation. This last process, if it were introduced into the manufactory of Brozo, would produce the sulphate of magnesia of commerce at a very moderate price.

Which when *sulphated* by the acid vapours, may be lixiviated.

The magnesian earth useful for porcelain and pottery,

As the magnesian earth of Baudissero forms an excellent porcelain with silex, it presents besides an interesting subject for research relative to the fabrication of pottery. With this earth and a quantity of the argillaceous earth of Castellamonte sufficient to unite it into a paste, I formed some crucibles and capsules. These crucibles were exposed for 48 hours in the furnace of the glass-house of Po. The earths did not seem to have formed a sufficient union; nevertheless the hardness of the crucibles was such that they could not be affected by the file. Doctor Gioanetti, who is now engaged in manufacture of stone-ware pottery, will hereafter throw light on this subject.

and mixed with argil forms crucibles extremely hard.

I end this part of my memoir with observing that the trials which I made of this earth as an absorbent in veterinary medicine succeeded perfectly well.

Additions to the preceding Memoir, by the same.

The earth of Castellamonte is similar to that of Baudissero.

Farther researches which I made on argillaceous earths have given me to understand that the earth of Baudissero is not the only one known that consists for the most part of magnesia. The same kind is also found at Castellamonte, a large village near that of Baudissero.

M. Bertoline, doctor of medicine, one of the most eminent of my pupils, having repeated the detail of the experiments which we made at the general school of chemistry, invited us to essay a particular earth of Castellamonte, his country, which he thought would furnish the alumen which was sought for unsuccessfully in the earth of Baudissero; soon after, by the care of M. Onorato, surgeon of Castellamonte, who is the proprietor

prietor of the land where this earth is found, I received a large quantity of it, and we examined it comparatively with that of Baudiſſero.

The earth of Caſtellamonte, which was brought to us had nearly the ſame appearance as that of Baudiſſero; but when it is firſt dug up from the ground, it has on the other hand different external appearances, which ſeem to depend on the different degrees of decompoſition of the Corneen ſtone or Cacholong, which is found at Caſtellamonte as well as at Baudiſſero.

When firſt dug up appears different in colour.

The colour of this earth is a white inclining to blueiſh. In a maſs this earth is opaque; but when ſmall fragments of it of a minute thickneſs are examined, they have a ſemi-tranſparency.

In this reſpect it has a ſtrong reſemblance to horn; it is very ſoft, and may be cut with a knife like hard cheeſe. It is more unctuous to the touch, and a little more adheſive to the tongue than the earth of Baudiſſero.

Reſembles horn, cuts like hard cheeſe, more unctuous, and adheres more to the tongue than the firſt.

Treated with the acids, like the ſoft ſpecies of Baudiſſero, it becomes diluted, and then diſſolves, but has however a very remarkable difference, which is that it diſſolves in all the acids without the leaſt efferveſcence.

Does not efferveſce with acids.

It alſo does not yield the leaſt appearance of carbonic acid on expoſing it to the fire in cloſed veſſels furniſhed with syphons, which communicate with lime water.

This earth, like that of Baudiſſero, does not contain the leaſt trace of alumine or of oxide of iron.

It contains, ſimilarly to that of Baudiſſero, a little ſulphate of lime and muriate of magneſia, which may be ſeparated by lixiviation in water.

Conſiſts of the ſame ſubſtances as the firſt.

The remainder conſiſts entirely of magneſia and ſilex; but the proportion of this laſt is greater in it than in that of Baudiſſero. It may be computed at from 10 to 20 hundredth parts.

But contains more ſilex.

When this earth is kept in contact with the air its external characters change.

Changes its appearance on expoſure to the air,

Its colour becomes by degrees a dull white, the ſame as has been remarked of the earth of Baudiſſero.

to a dull white;

Its ſemi-tranſparence is loſt; its particles ſeparate, and in two or three weeks it is found to have abſorbed carbonic acid to that degree as to make as marked an efferveſcence with the acid

Loſes its ſemi-tranſparence, and abſorbs carbonic acid, ſo as to efferveſe with acids, and be-

comes like the earth of Baudiffero, with the fmall difference noticed.

acid as the earth of Baudiffero. In a word, it is completely the fame as this laft, with this fole difference, that phyfically confidered it is lefs compact, and becomes even friable, and chemically confidered it contains a little more filex.

It appears then to be well proved that the earth of Baudiffero and that of Caftellamonte are each a true native magnefia, mixed with a little filex. In the earth of Caftellamonte it is fufficiently demonftrated that it contains no carbonic acid when in the bofom of the earth; and that it only contains it when, after a long expofure to contact with the air, it can abforb it from the atmofphere. That of Baudiffero contains in truth carbonic acid, but the quantity is much inferior to that which it ought neceffarily to contain to be confidered a carbonate of magnefia; befides the earth of Baudiffero having been worked for a long time, and being thus in contact with the air, it is from the atmofphere it muft have drawn it, and that in proportion to the time it has been expofed; at leaft I have no doubt that if the earth of Baudiffero was dug up from a certain depth, no carbonic acid would be found in it.

Earth of Baudiffero has not acid enough to be carbonate of magnefia.

If dug from a fufficient depth would probably contain no carbonic acid.

Earth of Cafelette probably magnefian alfo.

I will conclude this addition to the memoir, by obferving that the earth of Mufinet at Cafelette, being produced by the decompofition of the fame Corneen ftone or Cacholong, ought alfo probably to be a magnefian earth; but I have not yet made any experiments on this earth; Doctor Bonvoifin, who has given the analyfis of it in its ftate of Cacholong and Hydrophane ftone, propofes in conjunction with me to repeat the analyfis of this ftone, in the true ftate above-mentioned, and in its earthy ftate; which fhall be the fubject of a particular memoir. *

The author propofes to analyfe Cacholong and Hydrophane, and write a memoir of the refult.

* The laft ufe which Mr. Giobert mentions for magnefian earth is of the moft confequence of the two, for as fulphate of magnefia is only ufed in medicine, the fale could not be fufficiently extenfive to produce much profit on a large fcale.

The ufe of this earth for pottery is the more deferving of notice, as it has hitherto been fuppofed that argil was alone proper for this purpofe; and though it was long known that magnefia is of a very refractory nature in the fire, Mr. Giobert feems to be the firft who thought of ufing it in crucibles; which is the more extraordinary, as the *lapis ollaris*, which derives its name from its property of ferving to make utenfils to bear the fire, is well known to contain a large proportion of magnefia.

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VI.

First Communication on an artificial Tan prepared fron Coal, chared Wood, refinous Subſtances, &c. Abridged from the Original of CHARLES HATCHETT, *Eſq. F. R. S.**

MR. HATCHETT firſt notices, that the natural tannin was firſt extracted from the matters which contain it, by Mr. Deyeux, who conſidered it as a ſpecies of reſin; that Mr. Seguin firſt diſcovered it to be the ſubſtance which in the proceſs of tanning renders animal ſkins inſoluble in water, and imputreſcible; but that Mr. Chenevix alone had noticed the effect of heat in giving coffee berries the power in decoction of precipitating gelaten. He then ſtates, that his experiments on lac, and ſome of the reſins having ſhewed him the powerful action of nitric acid on ſuch ſubſtances, induced him to try its effect on aſphaltum and jet; theſe with it formed a dark brown ſolution, and a precipitate, which by digeſtion in another portion of the acid became diſſolved, and on evaporation produced a yellow viſcid ſubſtance ſoluble in water and alcohol, and perfectly ſimilar to that obtained by ſimilar means from the reſins, excepting that when burned it had the colour of the fat oils. This reſult led to the ſuppoſition, that the dark brown ſolution was of the carbonaceous

First diſcovery of tannin by M. Seguin and Deyeux.

Reſult of experiments on ſolution of lac, induced others on aſphaltum and jet, with nitric acid.

As native magneſian earth would doubtleſs be of great uſe to the potteries of this country, it is a pleaſing conſideration, that it is extremely probable it may be found in England, as well as on the Continent; for not only ſteatites and other magneſian ſtones have already been diſcovered here, but that ſalt, which it is M. Giobert's principal object to manufacture, is the natural produce of this country, and therefore the neighbourhood of Epſom, which gives it its name, may well be ſuſpected of containing beds of an earth ſimilar to that of Baudiſſero.

There is alſo ſome reaſon to ſuppoſe, that this earth may be one of thoſe ingredients in china-ware, which the Chineſe endeavour to keep ſecret; indeed it is hardly probable they ſhould be ignorant of its uſe, in a country, where the fineſt earthern-ware has been manufactured in the greateſt perfection, from periods antecedent to the dates of the authenticated hiſtory of Europe, and where of courſe experiments relative to the compoſition of this article, muſt have been varied to the greateſt extent.—B.

* Philoſ. Tranſ. 1805.

matter

matter, and that the yellow precipitate was the essential part of the bitumens, which was confirmed by results from amber; several experiments were tried with various sorts of pit-coal, from all which the brown solution was obtained in abundance, but those which contained little or no bitumen did not yield the yellow precipitate.

Pit-coal treated in the same manner,

Process with the Coal.

100 grains of coal, in each experiment, were digested in an open matrass in a sand heat, with an ounce of nitric acid (of the sp. gravity of 1.40) diluted with two ounces of water, which when warm produced effervescence, and discharged much gas; after two days, a second, and sometimes a third ounce of the acid was added, and the digestion continued for five or six days, when nearly the whole was dissolved, except the precipitate which was constantly separated.

and charcoal.

Charcoal was next tried, which dissolved more readily than the pit coal, and left no residuum.

The several solutions from asphaltum, jet, pit coal, and charcoal, were evaporated to dryness gradually to prevent burning the residue, which from all were of a glossy brown substance, of a resinous fracture, and had the following properties.

Properties of the residua of these solutions.

1. They were speedily dissolved by cold water, or alcohol.

2. Their flavour was astringent.

3. Exposed to heat, they swelled much, and gave a bulky coal.

4. Their solutions in water reddened litmus paper.

5. And gave copiously precipitates from muriate of tin, acetite of lead. oxy-sulphate of iron, of a brown colour, except the tin, which was dark grey.

6. They precipated gold in the metallic state from its solution.

7. They also precipitated the nitrates of lime, and of barytes, and other earthy salts.

8. The fixed alkalis, and ammonia added at first, deepened their colour, and afterwards made them turbid.

9. They caused precipitates from glue or isinglass solutions in water, more or less brown according to their strength, which were soluble in cold and boiling water, so that in their essential

essential properties they proved similar to those formed by the varieties of tannin hitherto known, except that they contained no gallic acid or mucilage.

Animal coal from isinglass was also tried in the same manner, this dissolved very slowly, but left a little of the coal unchanged, its solution was of a deeper colour, and managed as the others described, produced similar effects with the reagents, except some difference in the colour of the precipitates; and also gave an insoluble precipitate from the solution of isinglass; by which the curious fact is proved, that one portion of the skin of an animal may be made to convert another into leather. Residuum of animal coal has all the same qualities, nearly,

Coak gave a solution resembling that of pit coal, but did not produce the same yellow precipitate. and of coak also.

These experiments shew, that the tanning substance is best procured from carbonaceous matter when it is uncombined with any substance but oxigen; which was confirmed by experiments on Bovey coal, Sussex coal, Surturband from Iceland, and deal saw-dust, which being dissolved in nitric acid, and evaporated, the residues dissolved in water, neither precipitated gelaten, or shewed any other signs of tanning matter; but when the same materials are charred, and treated as before described, they copiously produced the artificial tan; as did also teak wood, which Mr. Hatchett had proved to contain neither tannin or gallic acid in its uncharred state. Tanning substance best procured from carbon, uncombined with any thing but oxigen.

Mr. Hatchett had made several experiments on the slow carbonization of vegetable matters in the humid way, principally by sulphuric acid, occasionally diluted. Concentrated sulphuric acid poured on any resinous substance reduced to powder, dissolves it in a few minutes; the solution is transparent, of a yellow brown colour, and a viscid oil-like consistence, but after being placed on the sand bath, grows darker, evolves sulphuric acid gas, and at last becomes a thick liquid of an intense black. Carbonization of vegetable matter by sulphuric acid.

Sulphuric acid of the above strength poured on common turpentine dissolves it readily, if a portion of the solution is then dropped into cold water, a precipitate of common yellow resin is formed; if after another hour or two, another portion is treated in the same way, the resin produced is of a dark brown, and that thus formed from a solution that has stood Effects of solution of turpentine in sulphuric acid dropped into water.

five

five or six hours, is completely black. When the digestion is continued for several days, until no more gas is given out, the resin will be converted into a black porous coal, which does not contain any resin, if the experiment has been properly conducted. This coal was about 43 *per cent* of the resin used, and after being exposed in a platina crucible loosely covered to a red heat, still amounted to 30 *per cent*, and by the slowness of its combustion, and other circumstances, approached nearly the character of some mineral coals.

Products of this operation dissolved in nitric acid.

A portion of the coal, the black resin, brown resin, and yellow resin obtained from the turpentine described, and also some of the turpentine itself, were each dissolved in nitric acid, and reduced to dryness; the residua, which varied in colour from yellow to dark brown, according to the substance employed, were dissolved in water, and examined with isinglass and other reagents.

Their residues after evaporation are tried with gelaten, &c.

The solution from the turpentine residuum, that of the yellow resin, and the brown resin, did not precipitate gelaten.

Effects produced.

That from the black resin yielded a considerable portion of the tanning substance, and that from the coal afforded it in great abundance. Hence it appears, that these substances yielded artificial tan only in proportion to their conversion into carbon.

Various kinds of wood, copal, amber and wax, reduced to coal by sulphuric acid, yielded similar products on being treated with nitric acid.

Tan formed by alcohol.

Mr. Hatchett formed the artificial tan from the resins, and gum resins (such as common resin, elemi, assafœtida, &c.) when reduced to the state of coal from long digestion with sulphuric acid, by means of alcohol, without using any nitric acid: In the carbonized state mentioned, they are digested in the alcohol, a portion is dissolved, a dark brown solution is formed, which by evaporation yields a mass soluble in water as well as in alcohol, and which precipitates gelaten, acetite of lead, and muriate of tin, but produces only a slight effect on oxy-sulphate of iron.

Supposition relative to tan from peat.

The author supposes, that the tanning matter known to be evolved by peat in certain places, is effected by a process in some respects similar to the above, since if it was produced by its mere digestion in water, all peat would afford it, which is contrary to experience.

Mr.

Mr. Hatchett put his discovery to the test of real practice, having actually *converted skins into leather by the artificial tan* procured as described, but observes, that the production of this substance, for the present, must be considered only a curious chemical fact, not altogether unimportant, and not capable of economical application, though he hopes, that hereafter a process may be discovered for preparing this species of tan sufficiently cheap to enable tanners to use it in their business.

Leather made by the artificial tan.

Hope of its economical production.

There is reason to suppose, that it would be superior to common tan for this purpose, as it appears from experiments mentioned in Mr. Hatchett's second paper on the same subject (which will be given in the next number) that "solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c."*

Reason for thinking it superior to common tan.

VII.

Memoir on the Discovery of a Factitious Puzzolana, presented to the French National Institute, by M. DODUN, *Engineer in Chief of Bridges and Highways in France* †.

THE deposited dust of ancient volcanic substances, has been long used in Flanders, and the adjacent countries, as a substitute for the Italian puzzolana, under the name of *trass*, or *ashes of Tournai*.

Factitious puzzolana of Flanders,

M. Faujas has proved by decisive experiments, made by order of government, that certain lutulent eruptions of ancient volcanoes at Vivarais, had the same qualities as the puzzolana

that of Faujas.

* The peculiar tanning property of the water of certain peat bogs and morasses, may be otherwise accounted for, than by Mr. Hatchett's supposition, by the fact, that peat is not uniformly the production of the same vegetable substances: wherever heath, tormentil, and perhaps some other plants, are found in abundance, the peat water will have this quality; in the case of heath, at least, it cannot be doubted; and perhaps the peat which does not yield tan may owe this deficiency to the total absence of vegetables of this species.—B.

† *Journal de Physique*, Tom. 61.

that of M. Bagge. puzzolana of Italy, and might be used instead of it. M. Bagge, a Swede, is also known to have composed an artificial puzzolana cement, with a black, hard, and flatey schist; but until 1787, no one ever thought that the territory of France contained in abundance non-volcanic substances capable of taking place of the Italian puzzolana with economy and advantage.

M. Dodun discovered his by chance. The discovery which I here present has, like many others of great utility, been the effect of chance.

The habit of examining the nature of stone in its bed, which enables the observer to judge of its qualities at first sight, fixed my attention on an immense quantity of calciform fragments of iron ore, in beds of from eight to ten feet thickness, following exactly the parallelism of the slightly inclined declivities, in the neighbourhood of Castlenaudery. I perceived in the adjacent fields many substances of the same nature scattered over the surface of the earth, of violet, brown, and black colours, which from their appearance, had a perfect resemblance to compact lava, which seemed extraordinary in a country where there was no appearance of ancient craters, or of volcanic eruptions. These I soon found out had been brought to this state by serving as hearths, or enclosures to the fires kindled in the fields by the peasants, either for agricultural purposes, or personal convenience when they watch their flocks in winter; as I saw soon after many similar arranged by hand on one another for these purposes.

Saw great beds of ferrigunous oxides at Castelnaudery.

Observed burnt fragments in the fields like compact lava,

which similarity made him think them fit for puzzolana.

The similarity of these fragments to volcanic products excited my desire to form a cement from them, by treating them in the same manner as puzzolana earth. The great quantity of iron which these oxides seemed to me to contain, the abundance of their siliceous particles, and the alumen which evidently entered into their composition, their great weight, and their non-effervescence with acids, altogether made me presume, that the cement formed from them would bind under water, and my expectation was not deceived.

Convinced by long experiments of its superiority to the Italian.

Fifteen months successive experiments, to discover the proportion of lime which this oxide would absorb to harden in water, without cracking when in the air, have convinced me, that my factitious puzzolona had all the good properties of that of Italy, without its faults. At this time I determined to propose its use in the public works, and demanded that comparative experiments should be made between it and the

Proposed for use in the public works.

the Italian puzzolana, in presence of the Commissaries of the Province of Languedoc, and of the Directors of the canal which joins the two seas. Great blocks of *Beton* composition made with both cements, were thrown into the reservoirs adjacent to the lock of Saint Roch, at Castelmandery, being first plaistered over with the respective compositions. *and tried comparatively before the Commissaries of Languedoc. In the reservoir near the lock of St. Roch.*

Six months after, the water was drawn off from the bodies of masonry, and it was then seen that the factitious puzzalona had acquired a solidity at least equal to that of Italy. The plaister made with the Italian puzzalona was cracked and chapped, but that formed from the factitious kind had entirely preserved the unity of its surface. *Its superiority to the Italian sort.*

The states of *Etats* of Languedoc altogether convinced of the authenticity of this discovery, by the results of the comparative trials of both kinds of cement which they had seen, and by the certificates of their commissaries, and persuaded of the great advantage it would be to France, decreed in 1789, in their last meeting, that the factitious puzzolana should not only be used instead of the Italian in the works under their direction; but moreover, that it should be demanded in favour of the author of it, as a testimony of public gratitude, that government should authorize the free circulation of it every where. *Testimony in its favour from the states of Languedoc.*

The great consumption of this factitious puzzolana obliged me to extend its manufacture, I formed a partnership with the proprietor of the ground. The foundation of an establishment on a great scale was laid at the mountain itself where the materials were found. The works carried on in its vicinity were likely to farther reduce the cost of the article, which was already one half less than that of Italy, and the public were about to enjoy the advantages of this manufacture, when the revolution paralysed every thing. *Extensive works of it began. Stopped by the revolution.*

In 1791 I informed the constituent assembly of this discovery; the certificates which proved it, and the results of the experiments were deposited at the office; the matter was ordered to be examined by M. M. Pelletier and Berthollet, and the assembly considering, that this factitious puzzolana might be of the greatest use to France, decreed that 2000 francs should be granted to its author, which was paid accordingly. *The discovery declared to the constituent assembly. Approved of and rewarded.*

On

On this occasion the celebrated Mirabeau declared the discovery to be so valuable, "*that if it had not yet been made, public encouragement should be held out to excite it.*"

The Constituent Assembly wished to have numerous similar establishments set on foot in France, so well were they convinced of its national importance; but the misfortunes of the times prevented the execution of a project, which the grand Chief of the empire may easily realize, to the advantage of the country, whenever it seems good to him to do so.

The troubles of France retards the manufacture of it.

Researches on the amelioration of our cements, and particularly on the nature of the materials proper to form artificial puzzolana, led me to try the calcination of various schists, of the bitumenous, ferruginous, and argillaceous sorts.

Examination of different schists.

The black slatey schist of M. Bragge, so common in France, was not forgotten: It is almost the same as that which the elder M. Grathieu essayed at Cherbourg last year; but I have constantly found that these schists always contain too little iron. I perceived that their repulsion of the water was slow and feeble, and that their solidification in the water was owing to the good quality of the lime.

Contain too little iron for puzzolana.

I was thus obliged to recur to my quartzose oxides of iron, from their containing a greater quantity of ferruginous principles; and can aver with the skilful Faujas, that the puzzolanas owe their property of hardening in water solely to the ferruginous particles which they contain: of this I have had many proofs. This truth is farther demonstrated in the pudding-stones, the brescias, and generally in all the amygdaloides with a ferruginous base or cement.

Puzzolanas owe their qualities to the iron contained.

The theory of our cements is but little advanced; perhaps we take simple conjectures for proofs relative to them. We effect the regeneration of silex, and of the carbonate of lime; we know the acid gases which perform the principal part in the affair: but in this important work we have been long ignorant of the degrees of their reciprocal affinity, their quantity, and the mode of their respective combinations. Our knowledge on this matter is confined to a few facts.

Theory of cements little advanced.

Many experiments have proved to me that the puzzolana, which soonest forms a body in the water, is not fit to be employed in the open air, where it cracks and chaps in all directions. And that which is proper for the air, and which acquires

Two different preparations of the puzzolana.

quires and preserves its tenacity in it, sets but imperfectly in water. This difficulty, of which the Institute will perceive the cause, has obliged me to keep two sorts of the factitious puzzolana; on the reciprocal use of which a memoir of instruction will accompany the sale. The two sorts may be distinguished by their colour.

The factitious puzzolana proper for works under water, is of a reddish-brown. That which is fit for works exposed to the air, is a dark violet. The latter is used for terraces, the embankments of basons, for the composition of inclosures, or for light roofs. Bridges of a single arch may be formed with it; and I have seen it adhere so strongly to glazed tiles, that it was necessary to break the tiles to detach it. — One fit for water-works. Another for exposure to the air, and proper for terraces, roofs, and arches.

The puzzolana proper for constructions beneath the water, forms the most solid body in it. Three months after immersion it is an actual stone capable of receiving a polish. The lime in it is always regenerated into carbonate of lime in ten weeks. — Water puzzolana forms a stone capable of a polish.

When it may be thought by any one that he has been deceived as to the certainty of these effects, it will always be found, that he either has not observed the quantities directed of the puzzolana and the lime, or that he has used the reverse of that kind of the cement proper for the work. — Nullity of effect caused solely by mistakes of the operator.

I commonly used lime in the state of impalpable powder, slacked in Lafaye's manner, for works exposed to the air; and employed lime in the state of *putty*, for works which were to be covered with water. Sometimes I used lime in powder for the same work. This difference depends on the degree of goodness of the lime, on its greater or lesser richness, or its proportional poverty. Custom gives the advantage of knowing the different kinds on mere inspection. — Lime used in powder with its; and in putty.

The use of lime in powder appeared to me to merit a preference in the preparation of mortars or cements. I prepared my factitious puzzolana in a certain quantity as soon as I knew the proper proportion of the lime; and I had thus the advantage of being able to work it in troughs in the same manner as sulphate of lime. The whole was well mixed together and put into sacks; by which means the masons had nothing to do with the mixture of the articles (which is too often left to unprincipled workmen); and being thus master of the respective — Advantages of getting the materials ready mixed.

ſpective proportions of the puzzolana and the lime, I could always be aſſured of the ſolidity of my cements.

Exterior characters of the iron oxides uſed.

There remains for me to deſcribe the exterior characters of the quartziferous ferruginous oxides, which form the baſis of my factitious puzzolans, and to relate the analyſis of them which I made about 18 years ago. I will content myſelf with offering the comparative reſults with the Italian puzzolana, both in the dry way and the moiſt.

Exterior Characters of the quartziferous Oxides of Iron.

Slight calcination changes them from brown to red. A greater changes them to a deep brown.

Their colour is of a reddiſh-brown before calcination, or ſlightly violet. A light torrification gives them a clearer red tint or a deep violet: one more intenſe renders them of a deep brown or of a violet-brown inclining to a black. The degree of the calcination for uſe is confined to thoſe two ſtates.

Long continued heat renders them black and porous like lava.

Urged at a longer continued heat, the colour becomes a deep black, then the ſubſtance becomes porous, entirely ſimilar to certain lavas of our modern or ancient volcanos, with which it is then difficult not to confound them.

Their fracture grained and earthy, and they contain quartz cryſtals.

Their fracture is grained and a little earthy, and ſmall cryſtals of quartz may be diſtinguiſhed in them by the naked eye, and almoſt always angular fragments of gray or milky quartz; a powerful magnifying lens cauſes in ſome fragments the diſcovery of needles of ſchorl, the amphibole of Hauy, and ſome ſmall tourmalines.

Needles of ſchorl, amphibole, and tourmalines.

Their ſmell argillaceous.

Their ſmell is ſtrongly argillaceous on breathing on them with the mouth.

Give no ſparks.

There is no fire produced by the uſe of the ſteel, when it does not ſtrike a quartoſe particle.

Do not efferveſce.

They do not effervefce with acids either cold or hot.

Are affected by the magnet.

The magnet acts a little on theſe oxides before calcination, and ſtrongly, or perceptibly, after it.

Weight of a cubic foot.

The medium weight of a cubic foot is 125°; that of the Italian puzzolana is but 91°.

Analyſis by the moiſt Way.

Analyſis in the moiſt way of the iron oxides,

I ſhall not weary the aſſembly by a detail of the manipulations relative to the ſolvents and re-agents which art uſes for the decompoſition of bodies, and ſhall only ſay, that ſilex, iron, alumen, and a ſmall portion of manganeſe, are the conſtituent parts of theſe oxides.

I repeated

I repeated these experiments many times, and had for a medium result from an hundred pounds, chemical weight,

50 parts of silex;
31 —— of iron;
16 —— of alumen;
3 —— of manganese, and loss.

100

If this analysis be compared with that of the puzzolana of Italy, which contains in 100 parts— *and of the Italian puzzolana.*

50 of silex;
25 of alumen;
16 of iron;
3 of lime;
6 of loss;

100

their respective properties may be appreciated according to the proportions of their integrant parts.

The excess of alumen causes the plaisters made from the Italian puzzolana to crack and chap in the open air: this fault arises from their great oxidation. I have been able to replace in them those principles which they lost by decomposition. *Excess of alumen causes the Italian kind to crack.*

Analysis in the dry Way.

I endeavoured to obtain a regulus from these oxides of iron by using a violent heat. I followed the process of Kirwan for the fusion of siliceous and argillaceous ores of iron; yet I never obtained a single metallic button; and only found at the bottom of the crucible a vitrified mass of an opaque black, or a scoria in the state of crude cast iron. *Analysis in the dry way by Mr. Kirwan's processes; afforded no metal.*

Desirous to know if I could procure a malleable button by using the blowpipe, taking borax for the flux and supporting the oxide on charcoal; I still could only obtain a spongy ingot resembling crude cast iron, and breaking both when hot and when cold. *The blowpipe tried unsuccessfully.*

Being placed on a support of glass (according to my method published in the Journal de Physique, Tome XXXI. pages 116 and 139), the oxide fused at the second attempt, the sup- *The oxide fused on glass.*

 port

port was coloured green, and ſmall grains of iron were ſeen to paſs firſt of a dark-green colour, then of a bright green, and afterwards to diſappear in evaporating. There remained on the globule only a ſlight tinge of blackiſh-green.

General reſult.

The reſult of all theſe facts ſeems to be, that this oxide is entirely deprived of its metallic principle, and that its ſuper-oxigenation renders it reducible and refractory.

The oxide may be uſed as a pigment.

The arts may draw ſome advantage from theſe oxides, by uſing them in pigments for buildings. I ſucceeded after many waſhings, in extracting from them a beautiful brown-red colour equal to that of commerce, and applied it to uſe ſucceſsfully.*

* This paper has been abridged in its introduction, in the details relative to negociations with the Conſtituent Aſſembly, and in ſome other points a little irrelevant to the puzzolana; but all matters directly tending to illuſtrate its nature and properties have been carefully copied.

M. Dodun's diſcovery may be of ſome uſe to this country, as there are in many parts of it large maſſes of iron-ſtone, and ſome is found in the vicinity of moſt coal-mines.

It has been long known that iron ochres have the ſame property of forming puzzolana with lime, when properly roaſted, and this circumſtance is mentioned at large in Chaptal's Chemiſtry. A patent has alſo been obtained in this country for the application of iron pyrites to the ſame purpoſe, the right to which was purchaſed long ago by Mr. Samuel Wyat. But the novelty of M. Dodun's diſcovery is, that poor iron-ſtone is equally fit for this uſe, as the other ſubſtances mentioned, which is of the more importance as it is very plentiful, and may often be procured in ſituations where the others cannot.

It may not be amiſs to mention here, that baſalt treated in the ſame manner, has the ſame property as the puzzolana: the whin-ſtone, of which the ovoidal paving-ſtones conſiſt moſtly, is of this kind; and it is found in great abundance in theſe countries, in different forms.—B.

Experiments

VIII.

Experiments and Observations upon the Contraction of Water by Heat at low Temperatures. By Thomas Charles Hope, *M. D. F. R. S. Ed. Professor of Chemistry in the University of Edinburgh. From the Edinburgh Philosophical Transactions, for 1804.*

To the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for some degrees of temperature above it, it seems to afford a very singular and curious exception.

Expansion of water by cold, &c. near the freezing point.

The circumstances of this remarkable anomaly have been for some time believed to be the following:

When heat is applied to water ice cold, or at a temperature not far distant, it causes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41st degree. Between this point and the 42d or 43d, it suffers scarcely any perceptible change; but when heated beyond the last-mentioned degree, it begins to expand, and increases in volume with every subsequent rise of temperature.

General or usual statement of the facts.

Ice-cold water contracts by increase of temp. to about 42°, and then expands;

During the abstraction of caloric, the peculiarity in the constitution of water equally appears. Warm water, as it cools, shrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then suffers a loss of two degrees without any alteration of density. But when farther cooled, it begins to dilate, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as soon as the water reaches the 32°, or after it has descended any number of degrees below it.

and suffers the reverse change by cooling.

Supposing this peculiarity of water to be established, it must appear, indeed, a very odd circumstance, that heat should produce contraction in this fluid, while it causes expansion in other bodies *; and no less strange, that within one range of tempera-

This supposed peculiarity has

* Is this mode of change peculiar to water?—I do not know of any experiments with other fluids, except that mentioned on page 343. Perhaps it may be common to all, or at least to all those which expand by congelation. Decisive trials of this point are the more desirable, because some of Count Rumford's general inductions require or suppose that seawater should not be thus affected.—N.

 ture

ture it ſhould contract, and in another expand, the very ſame ſubſtance. Before a deviation from ſo general a law ſhould be received as matter of fact, the proofs of its exiſtence ought to be clear and indiſputable.

been hitherto deduced from experiments in narrow necked veſſels,

The experiments hitherto publiſhed, from which this ſingularity has been deduced, have all of them been performed upon water contained in inſtruments ſhaped like a thermometer glaſs, and conſiſting of a ball with a ſlender ſtem; and the expanſive or contractive effects of heat and cold have been inferred, from the aſcent or deſcent of the fluid in the ſtem.

of which the capacities alſo vary by change of temperature.

To ſuch experiments it has been objected, that the dimenſions and capacity of the inſtrument undergo ſo much change, from variation of temperature, that it is difficult, if not impoſſible, to determine how much of the apparent anomaly ought to be imputed to ſuch changes, and that it is not improbable that the whole of it may be aſcribed to them.

The author ſhows the effect by other means.

The object of this paper, which I have now the honour to read to the ſociety, is to prove by a ſet of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water preſents itſelf as that ſtrange and unaccountable anomaly which I have already deſcribed.

Previous hiſtory.

It is worth while, before detailing my experiments, to give a ſhort account of thoſe obſervations which led to the diſcovery of the fact, and which in ſucceſſion have extended our knowledge of it, as well as of thoſe obſervations which have at different periods been offered to diſcredit, and to bring it into doubt.

Dr. Croune firſt obſerved that water appears to expand before it freezes.

The firſt obſervation relative to this ſubject was made by Dr. Croune, towards the cloſe of the 17th century, while engaged in inveſtigating the phenomena of the great and forcible, though familiar, expanſion which happens to water at the inſtant of freezing; a matter which occupied in a conſiderable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that inſtitution.

His narrative. The experiment ſhewed that water roſe in a long necked veſſel by cooling:

I ſhall relate in his own words his firſt obſervation: "I filled a ſtrong bolt-head about half-way up the ſtem with water, a day or two before the great froſt went off, marking the place where the water ſtood; and placing it in the ſnow on my leads, while I went to put ſome ſalt to the ſnow, I found it above the

the mark so soon, that I thought the mark had slipt down, which I presently raised to the water, and as soon as ever I mixed the salt with the snow, the water rose very fast, about one-half inch above it. I took up then the glass, and found the water all fluid still: it was again set down in the salt and snow; but when I came about an hour after to view it, the ball was broke, and the water turned to hard ice, both in the ball and stem *."

From this experiment Dr. Croune drew the conclusion, that water, when subjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr. Hooke immediately expressed strong doubts, and ascribed the ascent of the water in the neck of the vessel to the shrinking of the glass occasioned by the cold.

Whence he inferred an actual expansion. But Dr. Hooke ascribed the effect to the vessel.

To obviate this objection, and to preclude, as far as was possible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immersed in a mixture of salt and snow, and into it, when cooled, was poured, to a certain height, water previously brought to near the freezing point. The water began instantly to rise as before, and when it had ascended about one-fourth of an inch in the stem, the vessel was taken out, the whole water remaining fluid.

Dr. C. repeated his exp. with the same event, in a vessel previously cooled;

These experiments, supported by others of a similar nature, communicated by Dr. Slare to the Society on the 20th of the same month, appear to have satisfied its members, in general, of this fact, that water, when on the point of congealing, and while still fluid, is actually somewhat dilated previous to the remarkable expansion which accompanies its conversion into ice.

which gave satisfaction;

Dr. Hooke, however, continued unshaken, and retained the doubts he had expressed.

but not to Dr. Hooke.

Remarkable as the fact, as now stated, must have appeared, it seems not to have excited particular attention, nor to have solicited more minute examination; and indeed though philosophers did not lose sight of it, yet for near a century no one investigated it more carefully. Mairan, in his treatise on ice in 1749, and Du Crest in his dissertation on thermo-

* Birch's *History of the Royal Society*, Vol. iv. p. 263.

meters

meters in 1757, appear to be well aware of this property of water, but it is to M. De Luc that we owe the knowledge of the leading and more interesting circumstances, (*vide Recherches*, &c. 1772.)

Modern experiments of De Luc.

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the investigation, while engaged in ascertaining the phenomena of the expansion and contraction of different fluids by heat and cold.

He used thermometer glasses, and found the water to descend by cooling till 41°, and then rise till freezing;

He employed in his experiments thermometer glasses; and the included water, at or near the term of liquefaction, descended in the stem, and appeared to him to suffer a diminution of bulk by every increase of temperature, till it arrived at the 41st degree. From this point its volume increased with its temperature, and it ascended in the tube. This fluid, when heated and allowed to cool, seemed to him to contract in the ordinary way, till its temperature sunk to the 41°, but to expand and increase in volume, as the temperature fell to the freezing point.

The density of water, he thence inferred, is at its maximum at 41°, and decreases with equal certainty whether the temperature is elevated or depressed.

so that its density at 50° and at 32° appears the same.

M. de Luc says, indeed, that very nearly the same alteration in volume is occasioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increase or of diminution; and consequently that the density of water at temperature 50, and at temperature 32°, is the same.

His theory.

This philosopher did not conceive that the constitution of water, in relation to caloric, undergoes a change at the temperature of 41°, such that short of this degree caloric should occasion contraction, and beyond it expansion. He imagined that heat in all temperatures tends to produce two but quite opposite effects on this fluid, the one expansion, the other contraction.

In low temperatures, the contractive effects surpass the expansive, and contraction is the consequence: In temperatures beyond 41°, the expansive predominate, and the visible expansion is the excess of the expansive operation over the contractive.

In

In 1788, Sir Charles Blagden added the curious observations, that water, which by slow and undisturbed refrigeration permits its temperature to fall many degrees below its freezing point, perseveres in expanding gradually as the temperature declines; and that water having some muriate of soda or sea-salt dissolved in it, begins to expand about the same number of degrees above its own term of congelation that the expansion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philosophical Transactions*, 1801), he, or rather Mr. Gilpin by his direction endeavoured to ascertain, by the balance and weighing bottle, the amount of this change of density caused by a few degrees of temperature. Sir Cha. Blagden's obs. that water may be cooled many degrees below 32° without freezing, and continues to expand.

Every one must be familiar with the use which Count Rumford has made of this peculiarity in the constitution of water, in explaining many curious appearances that presented themselves in his experiments upon the conducting power of fluids, and in accountiug for certain remarkable natural occurrences. The Count, with his usual ingenuity, has endeavoured to point out the important purposes which this peculiarity serves in the economy of nature, and to assign the final cause of so remarkable an exception from a general law. Count Rumford's applications of this doctrine to the economy of nature.

In recording the observations and opinions that have been published concerning this point, I might now, in order, notice those of Mr. Dalton of Manchester, related in the fifth volume of the *Manchester Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr. Dalton himself has called in question the accuracy of the conclusion which had been drawn from his experiments, and from those of preceding observers, I shall only remark, that they are of the same nature, and nearly to the same purport, as those of M. de Luc. Mr. Dalton's experiments,

It was in consequence of a communication with which Mr. Dalton favoured me, three months ago, that my attention was directed to this subject. He informed me, that after a long train of experiments he was led to believe that he, and his predecessors in the same field of investigation, had fallen into a mistake with regard to the contraction of water by heat, and its expansion by cold, in consequence of overlooking or underrating the effect which the change in the capacity of the thermometer-shaped apparatus employed, must occasion on the apparent volume of the fluid. He stated, in general terms, that who questions the truth of the conclusions,

because the point of greatest apparent density is different in different vessels; viz. in earthen-ware at 34°, glass 42°, brass 46°, and lead 50°.

that on subjecting water to different degrees of temperature, in instruments made of different materials, he found the point of greatest density was indicated at a different temperature in in each.

In an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glass at the 42d; of brass at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the substance of the instrument, he conceived that the appearance of anomaly in this fluid originated entirely in the containing vessel, which must cause the fluid in the stem to fall or rise according as its expansions are greater or less than those of the included liquor.

A detail of these important experiments has, ere now, been transmitted for publication in the *Journals of the Royal Institution of London* *.

Mr. Dalton supports Dr. Hooke.

I have already noticed that Dr. Hooke endeavoured to explain in the same manner the original experiment of Dr. Croune. This explanation apparently gathers much force from these experiments of Mr. Dalton.

De Luc and Blagden were attentive to the vessel;

It is proper, however, to state, that M. de Luc was perfectly aware of the alteration in the dimensions of his glass apparatus, but deemed the change too trifling to have any material influence.

Sir Charles Blagden paid greater attention to the circumstance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

and various reasons afford ground for doubt.

When it is considered, that the whole amount of the apparent change is but very small, and that the expansibility of the glass is with difficulty ascertained, and is variable by reason of the fluctuating proportions of its heterogeneous constituents, it must be acknowledged, that precision in such a calculation cannot possibly be attained, and can scarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr. Hooke, and in some measure liable to the objection which he had urged. I confess, that the experiments of Mr. Dalton, in perfect concurrence with that explanation,

* They were transmitted to our Journal by the author in Vol. X, page 93.

created

created considerable doubts respecting the existence of the peculiarity of water; against the probability of which circumstance, all analogical reasoning, and every argument *à priori*, strongly militate.

Unwilling to remain in uncertainty, and considering it as a point of much curiosity and interest, I have endeavoured to investigate the subject by experiments conducted in a totally different manner, equally calculated to exhibit the singular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimensions of the instrument, should have any influence whatever.

The author's experiments were not made uncertain by the causes before stated.

I have already taken occasion to state, that the purpose of this paper is to prove, by experiments on the principle now mentioned, that in the constitution of water there really exists the singularity often noticed.

I shall first state the plan of the experiments, and then detail the particulars of the most remarkable of them.

When any body is dilated, whether by heat or cold, it necessarily becomes less dense, or specifically lighter; and the opposite effects result from contraction. This is the circumstance, as every one knows, which causes various movements among the particles of fluids, when any inequality of temperature prevails in the mass; hence these particles are little acquainted with a state of rest.

His attention was directed to shew whether water rises or sinks by the preceding changes of temperature;

If a partial application or subtraction of heat produce an inequality of density in a mass of fluid, the lighter parts rise to the surface, or the denser fall to the bottom.

It readily occurred, that I might avail to myself of these movements, and upon statical principles determine the question in dispute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to observe, by means of thermometers, what situation the warmer, and what the cooler parts of this fluid affected.

which could be done by thermometers duly placed.

If I should find that ice-cold water, in acquiring temperature, showed, in its whole progress, the warmer parts near the top, it would indicate that water follows the usual law, and is expanded like other bodies by heat.

Or if I should observe that warm water, in cooling to the freezing point, had the coldest portion uniformly at the bottom, the

For the cold portions of water during

change of temperature would constantly be at the bottom, if densest, throughout such change.

the same conclusion would follow; while a different inference, and the existence of the supposed anomaly, would be deducible should the event prove different. The only circumstance, I can figure to myself as tending in any measure to render this mode of examining the point doubtful, is, that water near its congealing point may have so little change of density occasioned by a small variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent mass, from assuming that situation which their specific gravity would allot to them.

It will appear, however, very clear, from the circumstances of the experiments which I shall immediately detail, that no obstacle to the success and precision of the experiments proceeded from this source.

It is not necessary for me to relate all the experiments I have made. I shall restrict myself to the detail of six, which present varieties in the modes of procedure, and which afford the most striking results.

Exp. 1. Ice-cold water exposed to a warm atmosphere, was warmer ($1\frac{1}{2}°$) below till 38°, after which it was warmer at top.

Exp. I. I filled a cylindrical jar of glass $8\frac{1}{2}$ inches deep, and $4\frac{1}{2}$ in diameter, with water of temperature 32°, and placed it on a table, interposing a considerable thickness of matter possessed of little power of conducting heat. I suspended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the same distance below the surface. The jar was freely exposed to the air of the room, the temperature of which was from 60° to 62°.

The experiment commenced at noon:

	Top Thermom.	Bottom do.
	32°	32°
In 10 minutes,	33+	34+
— 30 ———	35.5	37
— 50 ———	37	38+
— an hour,	38	38+
— ——— and 10 minutes,	42	38.25
— ——— —. 30 ———	44	40
— ——— —. 50 ———	46+	41+
— 2 hours and 10 minutes,	48	42.5
— ——— —. 30 ———	50	44
— ——— —. 50 ———	50.5	45
— 4 hours,	54	49

Confiding

Confiding in the indications of the thermometers, from this experiment we learn, that when heat flows on all fides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually defcend, and take poffeffion of the bottom of the veffel.

This downward courfe proclaims an increafed denfity, and teftifies that the cold water is contracted by heat. As foon, however, as the fluid at the bottom exhibits a temperature of 38°, this courfe is retarded and foon ftopped, and with the rife of temperature beyond 40° is totally changed; for when the mafs attains this degree, the experiment equally fhows, that the warmer fluid afcends and occupies the fummit, by its route announcing its diminifhed denfity, and proving that water is now expanded by heat.

Whence it is concluded that the colder (upper) fluid was rarer at the temperatures near freezing; and the warmer (upper) fluid was rarer at the higher part of the fcale.

Exp. II. I filled the fame jar with water of temperature 53°; and that I might obferve the phenomena of cooling, I placed it in the axis of a much larger cylindrical veffel, nearly full of water, of temperature 41°, and, by an earthen-ware fupport, raifed it about three inches from the bottom, taking care that the water fhould be on the fame level in both veffels. As foon as I had adjufted the two thermometers, as in the former experiment, I obferved that the top of the fluid was ftill at 53°, but the bottom had fallen to 49°.

Exp. 2. Water at 53° was every where cooled by enveloping the veffel with ice-cold water. It was warmer at top till 42°, after which it was warmer (4°+) below.

	Top.		Bottom.
In 9 minutes,	52°	-	45
— 15 ———	52	-	44

Now, to accelerate the cooling, I withdrew by a fyphon the water from the large cylinder, and fupplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes,	48°	-	42+
— 38 ———	44	-	40
— 43 ———	42	-	40
— 46 ———	40	-	40
— 52 ———	36	-	40
— 58 ———	35—	-	39
— 65 ———	34	-	37
— 75 ———	34	-	36
— 103 ———	34	-	34

This

This experiment is the counterpart of the foregoing, and from the teſtimony of the ſame inſtruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes poſſeſſion of the bottom of the veſſel, ſo as to eſtabliſh a difference of temperature from the ſurface, amounting ſometimes to 8°. And that as ſoon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that ſituation is ſtationary till the ſurface reaches the ſame point.

Whence the ſame concluſion is deduced as in the former experiment.

During the ſubſequent refrigeration, the progreſs of the cooling undergoes a total change. The thermometers tell that the colder fluid riſes to the ſurface; ſo that the top gets the ſtart of the bottom ſoon by 4°, and attains the loweſt temperature of 34° very long before the other falls to the ſame degree.

Theſe circumſtances, I think, lead to the concluſion, that by the loſs of caloric, water at 53° is contracted and rendered ſpecifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an oppoſite effect is produced; for now the water, as it cools, becomes ſpecifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the ſituation, which the warmer and colder parts of the fluid affected, in the progreſs both of the heating and cooling, while every external circumſtance of the proceſs continued unaltered, is particularly worthy of remark.

Exp. 3. A tall jar, nearly 18 inches high, containing water at 50°, was cooled round its *upper part* by ice and ſalt. The temperature fell quickeſt at bottom, till 40°, where it continued ſtationary; after which the ſurface ſunk to freezing, and congealed.

Exp. III. I took a glaſs jar, 17.8 inches deep, and 4.5 in diameter internal meaſure, having a neck and tubulature very near the bottom. I provided alſo a cylindrical baſon of tinned iron, 4.8 inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I ſecured this baſon, ſo that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the ſuperior portion of a cylinder of water, and it anſwered the purpoſe completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the riſing of the bottom, and having fixed it in this ſituation, I rendered the aperture water-tight, by a perforated cork and lute.

This

This very tall jar was placed on a table, with the interposition of some folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the bason, which embraced the top, a mixture of powdered ice and salt.

From time to time I explored the temperature near the surface, by inserting the bulb of a thermometer to the depth of half an inch nearly in the axis.

The experiment lasted 50 hours.

	Bottom.	Top.	Air.
One o'clock,	50°	50°	42°
In 11 minutes,	46.+	—	
— 15 ———	45	48	
— 21 ———	44—	46—	
— 31 ———	42	44	
— 41 ———	41	42	
— 51 ———	40 +	34	At this time a thin film of ice began to form in contact with the glass.
— 1 hour 6 min.	40	34	
— ——— 20 ———	39.5		
— ——— 44 ———	39.5		
— 4½ hours,	39.5		A crust of ice of some thickness now lined the glass, and air had fallen to 40°.
— 5½ hours,	39		
— 11 hours, *i. e.* at midnight,	39		Crust of ice complete.
— 19 hours, *i. e.* next morning,	39		Air 40°.
— 26 hours,	40		Air 40°. So much ice had melted that the cake was detached from the side of the vessel, and floated.
— 32 ——	40		
— 41 ——	40		Air 41°. Ice not all melted.
— 50 ——	41		Air 42°. Ice not entirely gone.

Review of the facts and remarks.

This long protracted experiment presents some striking facts, and its general import, with regard to the subject of investigation, agrees with the preceding. In it we see, that when the frigorific mixture abstracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared sooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a

 half

half inches distant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condensed fluid descending, and a corresponding one of a warmer temperature ascending. Now, if water observed the same law that other bodies do, and had no peculiarity of constitution, the same progress of cooling should continue. This, however, the experiment teaches us, is not the case: as soon as the fluid at the bottom exhibits a temperature of 40°, it ceases. The colder fluid remains at top, and quickly losing temperature, ere long begins to freeze. The continuance of the colder fluid at the surface surely denotes, that it is not more dense than the subjacent warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

As the fluid below 40° continued at top, it was not *denser* than that at 40°.

Did water observe the usual law, and lose volume along with temperature, this experiment, by its long duration, afforded ample time for the manifestation of it.

For not less than two days did ice-cold water maintain possession of the top, and for the same period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condensed fluid moved from the surface, to affect the inferior thermometer, or to attest the contraction of water by cold.

Yet the experiment does not show that it was *rarer*.

This experiment, however, I must remark, does not warrant the conclusion, that the water is actually expanded, though it in no degree opposes it. It proves no more, than that the contraction ceases at 40°; and that water of 32° is not more dense than of 39° or 40°. Nay, some may perchance alledge, that it does not prove so much; conceiving, that if at 40° the contraction, without ceasing altogether, becomes very inconsiderable, the difference of density occasioned by the subsequent reduction of temperature may be so very trifling, as not to enable the cold particles to take that situation which their gravity assigns to them, in opposition to the inertia and tenacity of the subjacent mass; and therefore that the colder, though heavier fluid, may be constrained to remain above. That this allegation should have no weight attached to it, the circumstances of the succeeding experiment will clearly show, as I shall soon notice.

It might be even alledged that a small excess of density prevailed;

but this is not entitled to regard.

Before quitting the consideration of the present experiment, it may be worth while to remark, that it may seem rather surprising,

That heat which passed by direct communication

prising, that the bottom of the fluid was not apparently affected in its temperature by the ice which so long occupied its surface. It might be expected, though no cold currents descended from above, that the caloric should be conducted from below, and that the temperature should by that have been reduced *. I suppose that the caloric did pass from the lower strata upwards, but extremely slow, by reason that fluids, as Count Rumford taught us, are excessively bad conductors of heat, and so very slowly, that the caloric entered from the atmosphere with sufficient quickness to prevent any depression of temperature below the 39th degree. must have been very slowly transmitted.

This experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to ascend."

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* ANNOTATION, BY THE AUTHOR, †

This experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count Rumford, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid state, seeing that for two days the fluid at the bottom of the vessel never fell below 39°, though the surface was at 32°. The opinion of Count Rumford that fluids cannot conduct heat from particle to particle,

From the circumstances detailed in his seventh essay, the Count concluded, that heat cannot descend in a fluid. From the present, it might with equal justice be inferred, that heat cannot ascend.

Had I not the fullest conviction that this celebrated philosopher has pushed his ideas too far, I might be disposed to consider this experiment as according well with the hypothesis. appears to be inaccurate.

Soon after the interesting speculations of the Count appeared, I began to investigate the subject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, satisfied myself that he assigned to fluidity a character that does not belong to it. Though since the date of these experiments, the public has become

† As this note subjoined at the foot of the page after the words *temperature should by that have been reduced*, in the original, is of such considerable length, I have taken the liberty of putting it in the same type as the text.—N.

become possessed of several series, well devised, and, in my opinion, of themselves conclusive, it may yet be worth while to state the tenor and result of them, by which the value of their testimony in favour of the conducting power of liquids may be estimated.

Experiments to shew that heat can descend in fluids:

The experiments were of two descriptions.

The one set, of the same nature nearly with those of Count Rumford, was designed to examine, Whether heat, when applied to the surface, can descend in a fluid; and the other to discover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the subjects of the Count's experiments, were employed for the first set.

To water (and to oil) the heat was communicated from the bottom of a metallic vessel (in contact with the surface of the fluid, and) heated by boiling water within.

To explore the conducting power of water and oil, the apparatus which I used consisted of two vessels of tinned iron, both cylindrical, and the one somewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the subjects of the trial, to different depths on different occasions. The smaller was ten and a half inches in diameter. By three hooks it was suspended within the larger pan, in such a manner, that the bottom of it exactly reached and came in contact with the surface of the fluid. This smaller vessel became the source of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the use of a syphon, all agitation and disturbance.

This hot vessel did not touch that which contained the fluid under experiment; and the containing vessel was kept cold round the surface of the fluid, and therefore did not carry any heat downwards.

In experiments of this nature, the difficulty is to prevent the conveyance of caloric by the sides of the vessel. I attempted, and, I think, I succeeded, in overcoming this difficulty, by encircling the larger vessel, as a height exactly corresponding with that of the surface of the fluid within, with a gutter or channel about half an inch in depth; and by causing a stream of cold water to flow constantly through a syphon into this gutter, while from the opposite side it ran off by a small spout.

The water was several degrees colder than the subject of the experiment; and keeping cool the portion of the vessel with which it was in contact, it intercepted the heat that would otherwise have travelled by this route to the bottom.

Mercury was tried in glass vessels.

For mercury I had recourse to vessels of glass.

In

In all the experiments a thermometer bore testimony that the caloric descended from the surface to the bottom of the fluid, and demonstrated, at least to my conviction, that fluids can conduct heat. — In all the experiments the heat descended

The progress of the heat, however, was very slow, and attested the important fact, for which we ought to be thankful to the Count—That fluids are very bad conductors. — slowly.

The second set of experiments was calculated to examine, in a very different manner, the position, That all interchange and communication of heat between the particles of fluids is impossible.

When a hot and a cold fluid are mixed together and well agitated, very soon an uniform is produced. This equality must proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count Rumford. To which cause it ought to be attributed, I conceived I might discover, by ascertaining whether, after such an intermixture, any separation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as soon as the agitation ceases, ought, by reason of their greater rarity, to accumulate, to a certain degree, at the surface, and there exhibit a temperature above the common one. — Other experiments of mixing fluids.

I first tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a stoppered glass jar, I shook them well for a short time. — When hot and cold water are mixed, they acquire a common temperature throughout, and never separate.

I then noticed the resulting temperature, and raising the ball of the thermometer towards the surface, I had an opportunity of observing, that it never rose the smallest portion of a degree above the common temperature which had been established.

I next made a similar experiment with alcohol, selecting it on account of its remarkable dilatability. I shook well, for half a minute, a mixture of equal parts of alcohol at tempe. 40° and at temperature 170°. The resulting temperature of the mass was 140°. — The same effect was found with alcohol.

Now, if this was a mixture of particles at 40° and at 170°, as the difference of specific gravity between the fluid at these temperatures is very considerable, some separation of the warmer and

and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never ascended above the point of equilibrium.

From these experiments I concluded, that the uniformity of temperature was established by an actual communication and interchange of heat between the particles.

Count Rumford remarked that the mixture might be too complete to allow of separation:

It may not, however, be improper to state, that Count Rumford, with whom several years ago I had the pleasure of conversing upon this subject, alledged, that the intermixture might be so complete as to prevent any separation whatever.

but oil and water do acquire the common temperature by mixture, and exhibit the same when separate.

If it be a property essential to fluidity, that heat cannot pass from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the speculation, the caloric is so communicated Though, *à priori*, I entertained no doubts respecting the result of the experiment, I poured a quantity of olive oil which had been heated by immersion in a vessel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by shaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the water; for when the fluids separated, and had arranged themselves according to their specific gravity, both of them had the same temperature of 78° *.

The experiments of the two descriptions now recorded, left on my mind little doubt that the Count had overstrained his conclusions.

Exp. 4. The tall jar of Exp. 3, containing water at 40°, was cooled round its *lower part* by ice and salt.

† *Exp.* IV. I took the same tall jar, and stopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan After suspending two thermometers, as in experiment first and second, I poured a mixture of ice and salt into the pan, to the depth of 1.2 inches, the air of the room being 40°, as in the last experiment.

The temperature fell as quickly at top as at bottom:

	Bottom.	Top.	Air.
Eleven o'clock,	40°	40°	40°
In 10 minutes,	38+	38+	

* This is also very strikingly the case with mercury and water.—N. † The text is here resumed.

In

	Bottom.	Top.
In 20 minutes,	38°—	38°—
— 30 ———	37—	37—
— 40 ———	36	36
— 60 ———	35.5	35.5
— 80 ———	35	35
- 100 ———	34.5	35
- 120 ———	34—	34
— 8 hours,	34—	34

A cruſt of ice began to form on the inſide of the glaſs when the water in the axis of the bottom and of the top was at 36°. In the courſe of the experiment, it became at leaſt an inch thick.

We learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as ſpeedily perceived at the ſummit as in the axis of that part, on the external ſurface of which it immediately acts. As fluids conduct heat ſo very tardily, this can only ariſe from currents of cooled water aſcending from the bottom, and theſe cold currents cannot move upwards, were not the water of them ſpecifically lighter than that of the incumbent warmer fluid.

whence it is concluded that the cooled water aſcended from the bottom from its greater rarity below 40°;

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, muſt be expanded by the loſs of caloric.

This experiment ſecures full force to the laſt, as it obviates the objection already noticed, and alſo precludes another. I have already ſtated, that it may perhaps be alledged, that the fluid at the top, in experiment third, though cooled to 32° did not deſcend, becauſe below 40°, the contraction is ſo trifling, that it does not occaſion a difference of ſpecific gravity ſufficiently great to cauſe the particles to deſcend, when oppoſed by the inertia and tenacity of the fluid through which they have to [illegible]: or it may be conceived, that the deſcent is ſo tardy, that time is given to the ambient air or ſubjacent fluid to furniſh heat enough to raiſe the temperature of the deſcending ſtream, and by that arreſt it in its downward courſe.

and the objections to Exp. 3. are removed.

But from the particulars above recorded, it is manifeſt, that the change of denſity between the temperature of 32° and 40° is quite ſufficient to put into motion the particles, and to

 enable

enable them to overcome the obstacle arising from inertia and tenacity, and to withstand the arresting effects of atmospheric heat.

Though these experiments, and some others of a similar nature, carried conviction to my mind, and perfectly satisfied me respecting the reality of the anomaly of water, I determined to vary somewhat the mode of making the experiment, so as to obtain still more striking results.

Another experiment with a still taller jar, 21 inches high.

For the fifth experiment, I used an apparatus which consisted of a still taller jar. It was 21 inches high, and 4 in diameter. I adjusted at the middle of its height a perforated bason of tinned iron, 2 inches in depth, and 10 in diameter. As this bason embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what course the heated or cooled fluid should take.

Exp. 5. The last mentioned jar was filled with ice-cold water. Heat was applied to a zone of two inches near the middle by means of warm water in a circumambient vessel.

Exp. V. I filled the jar with water at 32°. I placed it upon several folds of thick carpet, previously cooled to the same degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments first and second. I then poured water of temperature 68° into the bason, and by means of a spout arising from the side of it, and a syphon connected with a reservoir of water at the temperature now mentioned, I renewed the contents of the bason frequently, but without causing any agitation.

The temperature rose below to 36° but remained unchanged at top: but after the lower water had attained 39° it became stationary, and the temperature at top soon rose to 48.°

	Bottom.	Top.	Air.
At commencement,	32°	32°	33—35
In 10 minutes,	35	32	
— 15 ————	36—	32	
— 20 ————	36+	32	
— 25 ————	37	33	
— 30 ————	38	33*	
— 38 ————	38+	33	
— 45 ————	39—	33	
— 50 ————	39+	44	
— 55 ————	39+	45	
— 60 ————	39+	48	

* From this time I charged the bason with water of temperature 88°, and renewed it frequently.

Nothing can be more decisive with regard to the question in dispute, than the particulars of this experiment. Heat is applied

applied to the middle of a column of ice-cold water. The heated portion has an equal ſhare of the column of cold fluid above it and beneath it. There is nothing to determine its courſe in one direction or another, excepting its actual change of denſity.

The thermometer evinces that the warm current ſets downwards, and carries the increaſed temperature to the bottom. There, this inſtrument indicates the ſucceſſive riſe of ſeveral degrees, before the ſurface indicates the ſmalleſt acquiſition of heat.

Whence it is deduced, that a *warm* current of water between 32° and 39°+ deſcended becauſe *denſer* and that when the temperature was more than 39+ the *warm* current aſcended becauſe *rarer*.

The inference is plain, that the cold water is contracted by the heat.

The change of the effect of heat is equally well illuſtrated by this experiment.

No ſooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its courſe, and, by aſcending, carried the increaſe of temperature very rapidly to the ſurface, ſo that it ſoon ſurpaſſed the bottom, and continued to riſe, while the other remained ſtationary.

Exp. VI. I filled the jar uſed in the laſt experiment with water of temperature 39½°, the air and the ſupport being at 39°. Diſpoſing the thermometers in the uſual manner, I introduced a mixture of ſnow and ſalt into the baſon.

Exp. 6. The ſame jar was filled with water at 39½° and a freezing mixture applied to the middle zone. The fluid at bottom was ſcarcely changed, but that at top froze.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	39°
In 10 minutes,	39+	38+	
— 25 ———	39+	36.5	*,* At this time
— 35 ———	39	36—	ice began to be
— 55 ———	39	35	formed on the
— an hour and 10 min,	39—	34+	ſide of the veſ-
——— 35 ———	39—	34—	ſel.
— 2 hours,	39—	33+	

This experiment ſpeaks in as decided language as the preceding. It ſhows that when a portion, in the middle of a column of water at temperature 39.5 is cooled, the colder fluid riſes, and does not deſcend through the warmer maſs, and preſents the unequivocal demonſtration, that water of temperature 39½° is actually expanded by loſing heat.

So that the water cooled below 39½ did riſe by expanſion.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give

relative

The general fact is, that heat causes ice-cold water to contract to 40°, and afterwards to expand.

relative to the subject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes specifically heavier,—that water of 40° when heated becomes specifically lighter,—that water above 40°, by the loss of heat, or by cold, is rendered specifically heavier; and that water below 40° is, by the same cause, rendered specifically lighter.

Such being the general import, the conclusion is irresistible, that heat, in low temperatures, causes water to contract, and at superior temperatures to expand. The opinion, therefore, is founded in truth, that water possesses a peculiarity of constitution in relation to the effects of caloric, and that it is, within a short range of temperature, an exception to the general law of "expansion by heat."

The greatest density lies between 39½° and 40°.

So far as I can judge from these experiments, I am disposed to believe that the point at which the change in the constitution of this fluid in relation to heat takes place, lies between 39½°, and the 40th degree.

I am not at present aware of any objection to the method I have followed in establishing this singular anomaly, and in removing any doubts which may have arisen from the unavoidable influence which the instrument must have in the mode of conducting the investigation that had previously been adopted.

These experiments shew the nature of the change, but not its amount.

The plan of operation above described, however, only ascertains the fact; it gives no data for ascertaining the amount of the anomalous effect of heat.

Whether the expansions and contractions be the same at equal intervals from 40°.

I have already stated, that M. de Luc alledged, that from the temperature of 41°, the expansion occasioned by cold was very nearly equal to that produced by the same number of degrees of heat; and consequently that water possesses the same density at any given number of degrees of temperature above and below 41°. The first experiments of Mr. Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itself to be cooled before it begins to freeze. From one circumstance that constantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that caused by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was less than what occurred during the cooling or heating

ing of the fluid through the same number of degrees above it; and I conceive that, when other circumstances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a measure of the difference of density.

Alcohol, when heated or cooled, presents, by reason of its greater expansibility, a greater difference of temperature in these situations than water; and upon the same principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depression.

As the concurrence of the testimony of the experiments above related with the general opinion, will probably remove every doubt respecting the matter of fact, it remains a very difficult problem for those who are fond of philosophical investigation, to explain how heat shall occasion in the same fluid, without producing any alteration of mechanical form of chemical condition, at one time contraction and at another expansion, and to reconcile the contractive effect to the conceived notions of the mechanism of the operations of this energetic agent.

It is a difficult problem to explain, how these contrary changes by heat are effected.

When heat causes expansion, it is imagined to act by inducing a repulsion among the particles of bodies, which, opposing and overpowering the cohesive attraction, causes the particles to recede.

The question stated.

In what manner, then, the addition of heat can occasion, or allow, the particles of water to approach each other, and how the subtraction of it can make them retire to a greater distance, I confess I can in no measure comprehend.

An explanation, abundantly plausible at first view, very readily suggests itself to every one who is aware of the great and forcible expansion which happens to this fluid at the moment of its congelation. It it stated by Sir Charles Blagden, in the paper already quoted.

Sir Charles Blagden's explanation; viz.

The remarkable dilatation which water experiences at the instant of being converted into ice, is very generally ascribed, and I presume very properly, to a new arrangement which the particles assume, determined probably by their polarity; by which one side of the particle A is attractive of one side of B, while it is repulsive of another.

As water expands in freezing by virtue of a new arrangement of the particles,

Now,

it is probable, that the arrangement and the expansion may begin before solidity ensues,

Now, if this polarity operates with so much energy as to impart almost irresistible expansive force at temperature 32°, it is reasonable to suppose that it may begin to exert its influence, though in a far inferior degree, at temperatures somewhat more elevated. The expansion, therefore, that takes place, during the fall of temperature from 40°, may be imputed to the particles beginning or affecting to assume that new arrangement which their polarity assigns them, in which arrangement these particles occupy more space than before.

and the contrary.

Again, when heat causes water of 32° to contract, upon the same principle, it may be conceived to operate, by counteracting the small portion of the disposition to polarity that survives the liquefaction.

I am afraid that we cannot rest satisfied with this explanation. We must not be deceived by the plausibility of it.

The state of perfect fluidity depends upon the circumstance, that the particles of any body admit of ready motion upon each other, and that the change of relative situation meets with little or no sensible resistance.

Objection. This advance towards congelation, ought to impair the fluidity:

Water certainly possesses fluidity in a great degree, and its particles must of course encounter but little resistance, as they glide the one upon the other. But if these particles shall begin to exert any degree of polarity, by which certain faces become more disposed to attach to each other than certain others, this tendency would necessarily oppose that indifference with regard to position, which is essential to fluidity, and of course must impair the fluidity, and induce some degree of tenacity or viscidity.

which does not appear to be the case.

To appearance, however, water at 32° has its fluidity as perfect as at temperatures considerably elevated. Unwilling to trust to appearance, where experiment might decide, I have attempted in various ways to ascertain whether the water suffers any sensible diminution in this respect while it is expanded by cold. The following method I deem the most correct.

Experiments with Nicholson's gravimeter.

For the purpose, I employed a gravimeter, the one contrived by Mr. Nicholson for discovering the weight and specific gravity of solids.

This is a convenient instrument, but, unfortunately, it is by no means so ticklish as a balance. Duly loaded, so as to be equiponderant with the water, in which it is plunged, Mr. Nicholson

Nicholson says, it is sensible to the 20th part of a grain *. The one I have, though its stem be slender, is scarcely sensible to less than two or three twentieths of a grain.

The want of sensibility in the gravimeter arises, in a great measure, though not entirely, from a certain degree of tenacity subsisting among the particles of the fluid; and any thing that tends to increase this tenacity, must, in the same proportion, augment this want of sensibility.

To ascertain whether any sensible change in the tenacity or fluidity accompanies the expansion of water by cold, which the theory requires, I examined the mobility of the instrument when immersed in water at different temperatures. I first plunged it into this fluid, heated to between 60° and 70°. Under due loading, which sank it to the mark on the stem, it was not sensible to a weight less than two or three twentieths of a grain.

It rose and fell in warm, and

I then tried it in ice-cold water, and found that its sensibility was in no perceptible degree impaired. The coldness of the water, it must be remembered, causes some degree of contraction of the gravimeter. This contraction cannot fail to render the instrument in some small measure more sensible, and, so far as it goes, to counteract the sluggishness produced by any increased tenacity in the fluid.

in ice-cold water with equal facility.

But as the body of the instrument is made of glass, the amount of the contraction must be very small, and the change of sensibility arising from it so very trifling, as certainly by no means to obscure such an effect as an increase of tenacity would occasion. I therefore with some confidence conclude, that the fluidity of the water is not sensibly diminished, and consequently that the polarity has not begun to exert any sensible influence; it can scarcely, therefore, be accounted the cause of the dilatation.

Whence the author concludes that the fluidity is not sensibly changed.

* Perhaps the difference of sensibility in my instrument, and that of the learned Professor, may have arisen from a difference of the diameters of the stems. Mine was of one-fortieth of an inch. It was well rubbed with a clean linen cloth, which rendered the surface equally disposed either to descend or ascend; and the instrument was not judged to be in equilibrio with the fluid, except when the surface about the stem was neither prominent nor depressed. This was easily known by the reflected image of the window frame, or other objects being seen close to the stem without distortion.—N.

Annotation.—W. N.

Sir Charles Blagden's theory of expansion of water by heat does not suppose that it should be less fluid.

It does not seem to me that Sir Charles Blagden's explanation does necessarily imply that the fluidity of the mass taken as a whole, should be sensibly impaired when tried by the application of a mechanical test. It might be impaired in the same manner as the water is affected by mixing small floating fragments of a solid along with it. When a saline solution which would become solid by cold, such for example as the sulphate of soda, is cooled below its point of congelation, the crystals will be differently formed according to circumstances.

Instance of crystallization differing according to circumstances.

If the fluid be gently shaken or made to oscillate, a shower of minute crystals will gradually fall through the fluid; and the whole mass will be a considerable time before the crystallization is finished; but if, instead of this method of agitation, the glass be scratched by a quill underneath the fluid, in Sir Charles Blagden's way, or if a small instrument, having a crystal of the salt adhering to it, be dipped into the solution, the crystals will radiate with great rapidity from that centre of perturbation, and in a few seconds the whole of the solution will become rigid.

Whence it is conjectured that the expansion of water by cold may arise from minute crystals of ice in the fluid.

This common and very striking experiment of chemical lecturers, seems to me to indicate at least a possibility that small crystals of ice may be formed and float distinctly from each other in water, at 40 degrees and lower: and I think the metals afford us a number of instances in which a considerable interval of temperature is found to be between the commencement of crystallization and the solidification of the whole mass. In pewterers solder the interval is not less than 40 degrees. This hypothesis of such disseminated particles of ice, which seems to be nothing more than an expression of Sir Charles Blagden's theory in different words, will explain why the colder water should be lighter;—namely, because it must contain more ice, and also why the expansion ought not to begin but at some definite temperature.

A measure of the greater or less fluidity of bodies is very desirable.

Though it does not appear to me that the theory of Sir Charles must necessarily imply a change in the mechanical resistance of water from what may be called rigidity; yet there are many other reasons why philosophers should be desirous of measuring the variations of fluidity in bodies: that is to say, the greater or less facility with which their parts are moved amongst each other.

Dr. Hope's trial may be modified probably by a

The ingenious attempt of Dr. Hope to ascertain this from the resistance made by a fluid to the perforation

ration of its surface by a cylindrical solid, is liable to the objection that it supposes the attraction or repulsion between the solid and the fluid to remain unchanged by variations of temperature; whereas the contrary seems most probable. The doctor's experiment must be grounded upon a position that the greater the depression or the greater the elevation of a fluid round a small cylinder partly immersed in it, the greater must be the resistance from imperfect fluidity. But these effects are evidently as much governed by the attraction or repulsion of the solid with regard to the fluid as by the resistance which the experiments are intended to measure. I have somewhere read that water clocks and other instruments for measuring time, by the passage of water through small holes, go slower in cold weather. This may arise from contraction of the hole, though my author ascribes it to imperfect fluidity. After some meditation on this problem it still appears to me to be surrounded with difficulties. Perhaps it may be one of the best methods to suffer the fluid to drop from a capillary syphon in different temperatures. I am disposed to think that the drops would be smallest and the whole quantity in a given time greatest when the fluidity was the most perfect, or at least when the adhesion of the particles of the fluid to each other was the least. But even here the attraction of the small capillary extremity of the tube from which the drop would fall would require to be considered; and on this account the method would be preferable (if so) to Dr. Hope's only because the repetition of a great number of drops or quantity of effluent water would give a greater degree of precision to the result.

change in the attraction or repulsion between the water and the stem of the gravimeter.

Water clocks are said to go slower in cold weather; because the water is less fluid.

Supposition that most water would drop from a capillary tube when it was most fluid.

Is it likely that the rope pump turned regularly a certain number of turns in a given time would raise more water when coldest and least fluid? If it did not might we not infer that the fluidity of water is not sensibly affected by change of temperature?

Will the rope pump shew a difference in the tenacity or fluidity of water hot or cold?

Observations

IX.

Observations on Turf, from the German Rathbeger fur alle Stænd. By DOCTOR COLLENBUSCH.

It is not very probable that a man placed beside a fountain of pure water should suffer himself to die of thirst through neglect of using it, or possessing food in abundance, should not appease his hunger with it; nevertheless instances of this kind are not wanting.

Wood fuel very scarce in Germany. Other matter may be substituted for it.

Every one complains in Germany of the scarcity of wood for fuel. It is known that substances have been found in other places which can supply its place, and that they have been formerly used here; but all this cannot induce any one to search for *turf*.

Ungrounded prejudices prevent the use of turf for fuel.

It is easily conceived that proprietors of woods, through the fear of having their profits diminished, should endeavour to perpetuate ancient prejudices, and to extend the opinion that the plague only ceased its ravages since the use of *turf* for fuel has been discontinued; but it is difficult to imagine that magistrates instead of encouraging the preparation of this fuel, should endeavour to prevent those from doing so, who wished to engage in it.

Used in Germany from the most remote periods.

It is very likely that the discovery of the use of *turf* as a combustible was first due to chance; and besides the use of this fuel in Germany has been continued from periods more remote than any written documents extend to.

Various erroneous opinions formed of its production and use.

The principal causes which have prevented the search after *turf*, are the erroneous opinions which have been formed of the manner in which it has been produced, of its preparation, and its use; some of which are as follow.

1st That turf is found in veins like metals.

Some think, for example, that *turf* has been formed at the moment of the creation, such as it is now found in the earth; and that there are veins of turf, as there are of iron, copper, tin, and other metals; but experience proves the falsity of this opinion, for there is found in almost all parts of Germany turf covered with more or less earth, (if only a proper search be made for it) beneath which layers of trees may be seen, which proves that there formerly were forests in the same places.

Others

Others believe that at the time of the deluge vast forests were overthrown, and afterwards covered with herbs, reeds, and other plants, and that these vegetables having rotted by degrees, became at last this black combustible mass resembling earth, which must have required an enormous quantity of vegetables, as plains of many leagues square are found covered with beds of it to the depth of more than 25 feet, beneath which trees are discovered of great hardness, and almost petrified.

2d That it was formed at the deluge.

Others imagine that it is more probable that the sea transported the materials of the turf from the western countries to the eastern, and covered with them the trees which are found buried beneath the turf. It is very true, that these trees have their roots turned towards the west, and their heads to the east. But then it is difficult to explain how this substance could be carried to countries distant from the sea, and even to the tops of the highest mountains in upper Saxony, on the *Brocken* and the *Alps*.

3d That it was transported by the sea from the west.

Many persons are of opinion that torrents and rivers have drawn together and deposited leaves and branches of trees on the low grounds, and that they have thus accumulated the constituent elements of the turf; but this cannot take place in countries in which no large rivers are found, nor on high mountains. The microscope clearly shews that turf, especially that kind which is from the surface of the earth, is composed of mosses, herbs, rushes, and other vegetables, and their roots strongly interlaced, of which the greatest part is changed into earth.

4th That it was washed down by torrents.

The microscope shews it composed of vegetable fibres.

Paper has actually been composed from turf, and the water which has settled in turbaries is used to tan leather, which proves that it is principally composed of vegetables. Chemical researches have also discovered in it a mineral resin which principally promotes its combustibility. It appertains then partly to the vegetable, and partly to the mineral kingdom.

Paper made from it and leather tanned by its water.

Turf may be produced artificially, by digging trenches 6 feet deep, and from 15 to 20 feet square; the trenches become filled with water, and produce the first year a green slimy moss, the second year this mossy vegetation covers the water to the height of two feet, and a great quantity of filaments are discovered in it mixed with leaves and flowers, in the third year a stratum is established, which attracts the dust and the seeds which

Turf produced artificially by sinking deep and wide trenches, which fill up by vegetation.

which float in the air, and engender a quantity of marsh plants, of reeds, and of herbs, which the fourth year become so heavy that they fall to the bottom. They then become compressed there, and by successive repetitions of this operation, all the trench becomes filled up in the course of 30 years; however this turf would probably require 100 years before it would equal the ancient turf.

Three species of it. Although this turf is always the same in its constituent parts, it nevertheless differs in having these parts variously mixed, which occasions its being divided into three species. The first comprehends the *surface turf*, and is the most common kind; it is found almost every where; but it contains in some places more combustible matter, which makes its colour vary.

1st The surface turf.

Found wherever water stagnates and is covered with weeds, This species is always sure to be found wherever places are discovered where the water stagnates, whether on plains, elevations, or declivities, in such a manner as to form a thick blueish crust, and deposits a yellow mud; or where the soil is covered with moss, reeds, rushes, or ridges; and if at the same time the feet of the passenger sinks into the loose soil, if the earth bends beneath his feet; if trees are perceived (which are commonly little pines or fir trees, or sometimes other kinds of trees,) covered with much moss, inclined to one side, and half rooted up at the other by the wind; in all these places turf will be found near the surface, and it is only necessary to remove the sod to perceive it. But this operation may be performed more quickly and easily with the English borer, which also will shew the depth of the bed.

and where trees covered with moss are half up-rooted by the wind.

To procure it the water should be drained off. To procure the turf, the water should be drained off, which is easy to do if the country is elevated or has valleys in its vicinity; but the operation is more difficult when the earth is level. As persons are not always to be found capable of taking the levels of ground, the places should be remarked where the water settled in spring when the snow melts; these places should be marked by stakes, and afterwards the trenches should be made to pass this way which are to be dug, to let the water run off.

Easy method of finding the descent for the drains.

Method of preparing the surface turf. To cut the turf an iron spade is used, which should be neither round nor pointed, but terminating in a straight line; this should be screwed down as far forwards as possible, along the side of a stretched cord, by a line 14 inches long and six broad; the detached part is separated from the depth of three inches

inches, at two strokes of the spade, to the length of 16 inches, and $4\frac{1}{2}$ inches broad, and this piece of turf is afterward divided in two.

In order that the pieces of turf may dry quickly, they should be placed on planks, and disposed so that the air might freely circulate between them, and that they could receive the rays of the sun. Method of drying it.

When the turf is thus dried to a certain degree, it is placed under sheds to compleat the drying; for if it was exposed to the sun till it was entirely dry, it would lose its strength and burn like straw. Should not be dried too much in the sun,

It is also disadvantageous to cut a large provision of it for many years, for the last made is always the best. The upper and lower beds are also observed to be of inferior quality to those in the midst; the best turf is that of a brown colour inclining to black, is heavy, and its texture is traversed by a small quantity of roots; this kind produces a strong and lasting fire, and its smell is very supportable. The more it is of a bright brown colour, the greater number of roots in it, and the lighter it is, the worse is its quality. This sort consumes more speedily, and may serve to advantage where a quick fire is wanted; its odour, it is true, is very disagreeable, but its ashes are excellent. or kept too long. Upper and lower beds of it the worst. Best kind dark brown and heavy. Bright brown or reddish worse.

The turf which inclines to a grey or yellow colour, and which is mixed with reed, is always the worst sort, but always good enough to heat kilns or ovens, and its ashes are good; this species is seldom found below the depth of two ells; it is reproduced after several years. Grey or yellow sort the worst.

The second species of turf is the *crumbling turf (moder-torf)*, this kind is found more abundantly in Holland; its cutting and preparation require much more pains than the surface turf. Second species, the crumbling turf.

The third species, or the *mountain turf*, is dug up from pits and galleries, and is reduced to regular forms like the preceding kind. Third species, the mountain turf.

It is objected to the use of turf that it cannot be employed as a substitute for wood in all the places where wood is burned; but it should not be forgotten that wood itself is not fit for every work where fire is required; that in order to be employed in founderies it must be charred with much trouble, and with a loss of two thirds of its weight, and that wood as well as turf is of different qualities and produces different effects. Wood not proper for fuel in all cases more than turf.

Turf

Turf may be charred. Turf is also susceptible of amelioration, especially the surface turf, the crumbling turf, and the mountain turf likewise; for it may be reduced to charcoal, and will thus serve for every work which requires fire; and in this case it yields neither smell nor smoke. The more strongly the turf is compressed before its carbonization, the more excellent is the charcoal.

Advantages from the use of turf. To compensate for the inferiority of turf to wood, granting that it is inferior, its use will prevent the great price which will otherwise necessarily be paid hereafter for timber for building, and will admit of the woodlands being proportionally reduced; *Cabbages, &c. may be planted where it has been extracted.* the places also where the surface turf has been dug up, if it has not been from too great a depth, may serve for situations wherein to plant cabbages, beets, and madder, or they will serve for fish ponds. *Its use admits of having more manufactories where fire is used.* The use of turf will admit of the multiplication of manufactories which use fire, of mines and forges; aged persons and children may be employed in preparing it; its ashes form a good manure, and the mould which falls from it may easily be converted into ashes.

X.

Experiments on the remarkable Effects which take place in the Gases, by Change in their Habitudes, or elective Attractions, when mechanically compressed. By THOMAS NORTHMORE, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

Devonshire Street, Portland Place,
Dec. 17, 1803.

SIR,

The author's reason for early publication. IT was my intention to have postponed troubling you with the following experiments upon the condensation of the gases, until I had brought them to a greater degree of perfection; but being informed that several of them have already, by means of which I am ignorant, and probably in a mutilated state, found their way to the press, any further delay seems improper. If then you deem the present communication worthy a place in your interesting Journal, it is entirely at your service.

It had long ago occurred to me, that the various affinities which take place among the gases under the common pressure of the atmosphere, would undergo considerable alteration by the influence of condensation; and the success attending the *violent* method adopted by the French chemists, which violence did not appear to me requisite, afforded additional encouragement to my undertaking some experiments upon the subject.

His suspicion that the affinities of the gases would be changed by compression.

I communicated this to the late chemical operator in the Royal Institution, a gentleman eminently conversant in the science, and with whom I was then engaged in a series of experiments: he not only approved of my design, but seemed to think it not improbable that an extensive field might thus be opened to future discoveries. Whether these opinions are justly founded, is now left for you, Sir, and the public to judge.

In entering upon a field entirely new, obstacles were of course to be expected: nor without reason; for though I had applied to one of the most eminent philosophical instrument-makers in London, Mr. Cuthbertson, yet I began to fear, even at the outset, that his skill would be set at defiance. The first instruments which he made for the present purpose were, a brass condensing-pump, with a lateral spring for the admission of the gas by means of stop-cock and bladder; two pear-shaped receivers, one of metal of the capacity of seven cubic inches, and another of glass of about three and a half: these were connected by a brass stop-cock, having a screw at each end. The metallic receiver was soon found to be of little or no utility, as well on account of its liability to be acted upon by the generated acids; its being too capacious, and thus consuming too large a quantity of gas: as because, though the result of an experiment might thus be known, yet the changes which the subjects might undergo would necessarily escape observation. The glass receiver obviated all these difficulties, and one or two imperfect experiments were performed with it; but the stop-cock speedily failed in its effect. For the power of the compressed gases was so great, partly from their elasticity, and partly (where affinities had operated) from their corrosive quality, as absolutely to wear a channel in the metal of which the plug was made, and thus to

Difficulties of the undertaking as to the instruments.

Condensing-pump.

Receivers.

Various objections.

to effect their escape. But not to trouble you any further with the obstacles that occurred, and which are mentioned only to prevent unnecessary expence to others) I have at last, by Mr. Cuthbertson's assistance, procured a connecting-tube, to which a spring-valve is adapted that has hitherto answered every purpose. *See Plate XIV. Fig. 2, 3, 4, 5, 6.*

Instruments now used by the author. Pump.

The instruments which I now use, are, 1st. An exhausting syringe; 2d. A condensing-pump, with two lateral springs for different gases; 3d. The connecting spring-valve; and lastly, glass receivers, which should have been of various sizes, but the one mentioned above having burst, that which I have principally used in the following experiments, is of about five cubic inches and a quarter in capacity, and made of glass well annealed and a quarter of an inch in thickness. Besides these instruments, I have occasionally applied Mr. Cuthbertson's double syphon-gage (*See Fig. 6*), by which the number of atmospheres condensed in the receiver, or rather the elastic power of the gases, may be measured; but this is rendered of less service, because a stop-cock must then be placed between the receiver and spring-valve, which frequently impairs the whole experiment; and also because, after a certain degree of condensation, and more particularly upon the admixture of the gases, new affinities usually take place, which tend to diminish the elasticity: the greatest number of atmospheres my gage has yet measured, is eighteen. These, Sir, with some bladders and stop-cocks, various iron screw-keys, and a wooden guard for the legs in case of bursting, constitute the principal part of the requisite apparatus.

Glass receiver.

Syphon gage.

Eighteen atmospheres compression.

I now proceed to the experiments, premising that the first four were made with the imperfect apparatus, when the gas was continually making its escape through the stop-cock.

Experiment I.

Exp. 1. Hidrogen, oxigen, and nitrogen gave water, and probably nitrous acid.

Into the glass receiver, of three cubic inches and a half capacity, were compressed in the following order:- Hidrogen, two (wine) pints; oxigen, two pints; nitrogen, two pints.* The result was, water, which bedewed the inside of the receiver; white floating vapours (probably the gaseous oxide

* These gases therefore occupied about five times the capacity they were condensed into.—N.

of

of nitrogen); and an acid which reddened litmus paper. Mr. Accum was present at this experiment, and from his opinion, as well as from succeeding experiments, I have reason to think that this acid is the nitric.

Experiment II.

As a difference of arrangement in the order of the gases tends considerably to vary the result, I repeated the former experiment (having first poured a little lime-water into the receiver) by injecting first the oxigen, about three pints, then equal quantities of hidrogen and nitrogen. Much of this gas escaped, owing to the imperfection of the instrument; but upon the affusion of the nitrogen, the white vapours again appeared in the receiver; water seemed likewise to be formed; and some yellow particles were seen floating upon the lime-water. These particles probably arose from the resinous substance, used in fastening on the cap of the receiver, being dissolved by the nitrous gas formed during condensation. I would just observe, that the magnet *seemed* to be affected during this experiment; but as there is iron used in the machine, this may be otherwise accounted for.

Exp. 2. The same, but the oxigen first.

Experiment III.

Two pints of carbonic acid, and two of hidrogen, were subjected to condensation. The result was, a watery vapour, and a gas of rather offensive smell.

Exp. 3. Carbonic gas and hidrogen. Water and a changed gas.

Experiment IV.

Trying to inflame phosphorus by the condensation of atmospheric air, the bottom of the machine (where it had been repaired) burst out with an explosion. This happened when I had immersed the apparatus in water to discover where the air escaped. The receiver was full of the fumes of the phosphorus, which was itself dispersed in the vessel of water. I afterwards repeated this experiment with the more perfect apparatus, but I could not inflame the phosphorus, and the fumes which arose at first soon disappeared. There was just enough acid (probably phosphoric) formed on the inside of the receiver to tinge litmus.

Exp. 4. Phosphorus in condensed air.

Experiment V.

Exp. 5. Repetition of Exp. 1. with better apparatus.

Having now the ſpring-valve, and new receiver of five cubic inches and a half capacity *, I poured in two ſcruples of ſolution of potaſh, and then injected two pints of hidrogen, two of nitrogen, and three of oxigen. This quantity was hardly ſufficient for the capacity of the receiver, and the reſult was only a ſmell of the gaſeous oxide of nitrogen, a few yellowiſh fumes, and ſcarce enough acidity to tinge the edge of the teſt paper: of courſe, I could not effect the formation of nitrate of potaſh.

Experiment VI.

Exp. 6. Nitrogen (firſt) and then hidrogen and oxigen.

I now determined to begin with the nitrogen, which always appeared to me to undergo the moſt important chemical changes, and therefore injected two pints of nitrogen, three of oxigen, and two of hidrogen. Upon the condenſation of the nitrogen, it ſpeedily aſſumed an orange-red colour, which upon the acceſſion of the oxigen, gradually diminiſhed, and at length diſappeared, though at firſt it ſeemed rather deeper. A moiſt vapour, coating the inſide of the receiver, aroſe upon the compreſſion of the hydrogen, which moiſture was ſtrongly acid to the taſte, coloured litmus, and, when very much diluted with water, acted upon ſilver.

Experiment. VII.

Exp. 7. The ſame, but different arrangement.

Nearly the ſame as the laſt, but with different arrangement. The nitrogen, three pints and a half, was firſt introduced; then the oxigen, two pints; and next the oxigen, three and a half. The nitrogen formed the orange-red colour as before; the hidrogen produced white clouds at firſt (*quære* ammonia?) which afterwards diſappeared, and the orange-red colour became lighter; but upon the affuſion of the oxigen, the colour did not diſappear as in the laſt experiment, but, if any thing, became darker. I then injected two pints more of hidrogen, but this had little or no effect upon the colour. Some vapour was generated, which was, as uſual, ſtrongly acid.

Experiment VIII.

Exp. 8. Nitrogen over lime-water.

Previous to the burſting of the ſmall receiver, I had put in it a ſcruple of lime, and condenſed upon it three pints of

* One fifth part of a pint very nearly.—N.

nitrogen.

nitrogen. The result was, a little reddish colour at first, which soon vanished. Upon repeating this experiment in the large receiver, I could produce no colour at all. In my present state of knowledge I am unable to account for this circumstance; but as soon as I get my new receivers of a smaller capacity, I mean to repeat the experiment.

Besides the above, I have made various other experiments with different gases, but I think it right to repeat them with greater accuracy before I submit them to the eye of the public; if upon that repetition they appear to me to be attended with results of sufficient importance to occupy a place in your Journal, I will take the liberty of communicating them to you, and am, Sir,

Your most obedient servant,

THO. NORTHMORE.

P. S. I think it necessary to add, that during the course of the above-mentioned experiments, there was a great variation of temperature in the atmosphere, from the heat of 70 degrees of Fahrenheit to the cold of 33.

Explanation of the Figures, by Mr. Cuthbertson.

Fig. 2, 3, 4, 5. Plate XIV. represents sections of the several parts of the spring-valve for the condensing syringe; *a* is a female screw, intended to receive the male at the end of the syringe; *b* is a square, to which is a key to screw it perfectly tight to the syringe: *d*, Fig. 3. is a female screw fitted to the male *c*; *e* is a male screw fitted to the female of the glass receiver; Fig. 4. is a round steel arbor, turned with a conical part and flat shoulder at *a*; *a b* is a spiral spring; Fig. 5. is a hollow brass cylinder serving as a cover and guide to Fig. 4; the piece Fig. 2. has a small hole drilled through the center, and turned out at the end *c*, so as to fill the cone *a*, Fig. 4; Fig. 3. is turned out at *f* so wide as to receive Fig. 5. Description of a valve for condensing.

If the plane shank of 4 be put into the hole *a c* till its cone shut close into the hollow cone at *c*, Fig. 2. and the other end with the spiral, covered by Fig 5, screwed tight upon the flat end of *c*, and *d* be screwed to *c*, all the joints being properly supplied with oil, and leathers, it is fitted for use.

Fig. 6. Represents a section of the condensing or double syphon gage, being a glass tube bent into the form of the figures,

figures, the end a is mounted with a brass screw, having a hole through it corresponding with the inside of the tube, the leg $b\ c$ is filled with mercury, and d is hermetrically sealed: $d\ c$ is divided into atmospheres.

XI.

Account of a Graphometer for measuring the Angles of Crystals. In a Letter from Mr. ROBERT BANCKS, *No.* 411, *Strand.*

To Mr. NICHOLSON.

SIR,

Great advantage of distinguishing minerals by their figure.

I NEED not point out to you, and to the learned readers of your Journal, how great the advantage will be, whenever the same may be realized, of distinguishing subjects of the mineral kingdom by their external appearance. This has long been done, with considerable precision, by operative men who have acquired their skill from continued practice, but without being able to communicate the knowledge they possess by any simple indications, such as might be given in writing, or through the medium of the press. Neither need I on this occasion point out how much we are indebted to the labours of Bergman, Romé dé l'Isle, and above all, Haüy, for scientific investigations of the forms of crystals, which at present bid fair to afford us criterions of the most extensive use. My present object is to communicate what I hope will be thought one step, however small, towards facilitating the admeasurement of their angles. In your first vol. at page 132, you have given an account of the graphometer of Carangeau, which is now considerably known and esteemed. I have rendered that instrument somewhat cheaper, and easier in the execution, and more correct in its use. For the sake of those who may not have that volume at hand, I shall briefly state, that the instrument consists of a semi-circle, like that which I am about to describe, and a pair of compasses or legs having their centre in the centre of the semi-circle, but capable of having their points drawn back, so as to admit of their application to any small crystals. The arc of the semi-circle is divided

Crystallography.

Carangeau's graphometer for crystals.

divided into two quadrants by an hinge, so that one part may be turned back out of the way of any mineral, which may require to be brought up towards the centre for admeasurement; and the same arc can afterwards be restored to its place, in order to shew the degree and fraction of the angle.

Improved graphometer. The semi-circle is entire, and the compasses or callipers are used separately for measuring the crystal, and applied to the semicircle for reading off.

In my improved instrument I avoid this joint, and obtain a much firmer framing by making my arc in the form of a protractor, as in Fig. 1. Plate XV. having an hollow centre at A, and a stud at B, both lying in the direction of that diameter which terminates the graduations. The compasses, or radii, or legs, are shewn in Fig. 2. separate from the arc. Their centre C is made like those of the common proportional compasses, and admits of the legs C D, C F being considerably lengthened or shortened when the two pieces are applied to each other. D E the fixed leg is represented as beneath F G the moveable leg or radius, and the lower end of the centre pin is made to fit the hole A precisely, at the same time that the stud at B being admitted into the long perforation towards E, the piece D E becomes steadily attached to the semi-circle, as is seen in Fig. 3.

The use is obvious. The crystal must be measured by the detached compasses as in Fig. 2, which are much more handy for all descriptions of minerals than Carangeau's entire instrument; and when thus set, if fig. 2 be applied to fig 1, as before directed, the angle will be read of at the fiducial edge of G.

I hope you and your readers will consider this as the useful simplification of a valuable instrument, and shall be happy to receive your sanction by its appearing in a work so generally known and esteemed as your Journal.

I am, Sir,

Your obedient Servant,

ROBERT BANCKS.

Nov. 1, 1805.

Accounts

ACCOUNT OF NEW BOOKS, &c.

Philosophical Transactions of the Royal Society of London for 1805. Port II. Quarto 353 pages, with an Index, and Six Plates. Nicoll.

Philosophical Transactions of the Royal Society.

THIS part contains the following communications, 1. Abstract of Observations on a Diurnal Variation of the Barometer between the Tropics. By J. Horsburgh, Esq. 2. Concerning the Difference in the Magnetic Needle, on board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, Esq. Commander of his Majesty's Ship, Investigator. 3. The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uses in different Animals. By Anthony Carlisle, Esq. F. R. S. 4. On an Artificial Substance which possesses the principal Characteristic Properties of Tannin. By Charles Hatchett, Esq. F. R. S. 5. The Case of a full grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, F. L. S. 6. A Description of Mal-formation in the Heart of an Infant. By Mr. Hugh Chudleigh Standart. 7. On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Esq. F. R. S. 8. On the Direction and Velocity of the Motion of the Sun and Solar System. By William Herschel, L. L. D. F. R. S. 9. On the reproduction of Buds. By Thomas Andrew Knight, Esq. F. R. S. 10. Some Account of Two Mummies of the Egyptian Ibis, one of which was in a remarkable perfect State. By John Pearson, Esq. F. R. S. 11. Observations on the singular Figure of the Planet Saturn. By William Herschel, L. L. D. F. R. S. 12. On the Magnetic Attraction of Oxides of Iron. By Timothy Lane, Esq. F. R. S. 13. Additional Experiments and Remarks on an Artificial Substance, which possesses the principal Characteristic Properties of Tannin. By Charles Hatchett, Esq. F. R. S. 14. On the Discovery of Palladium, with Observations on other Substances found with Platina. By William Hyde Wollaston, M. D. F. R, S. 15. Experiments on a Mineral Substance, formerly supposed to be Zeolite, with some Remarks on Two Species of Uran-glimmer. By the Rev. William Gregor.

Improved Graphometer by Mr Banks.

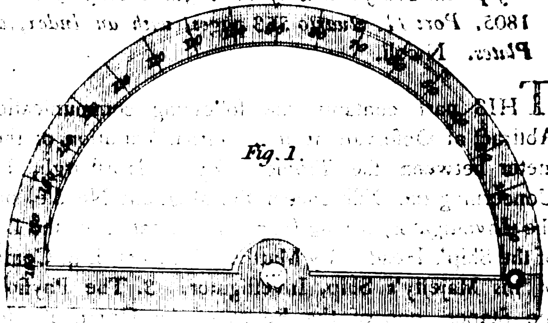


Fig. 1.

Fig. 2.

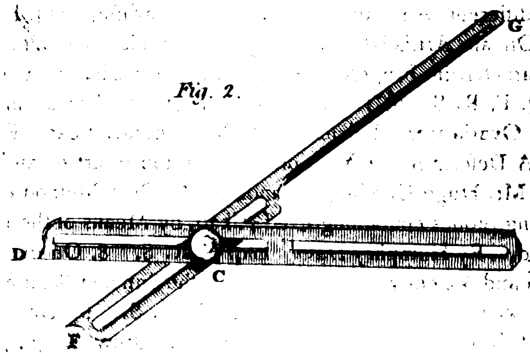
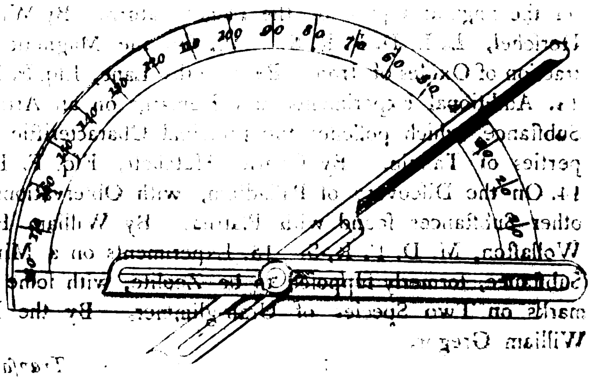


Fig. 3.



Mutlow Sc. Russell Ct.

Transactions of the Royal Society of Edinburgh (being the Continuation of Part II. together with Part III. of the Fifth Volume) Edinburgh Quarto 100 pages Continuation of Part II, and 126 Pages, Part III. No Plates. Transactions of the Royal Society of Edinburgh.

THE heads of memoirs and communications made to the Society since their last publication are disquisitions on the origin and radical sense of the Greek prepositions, by Mr. James Bonar, and experiments on the contraction of water by heat, by Dr. Thomas Charles Hope. These two papers of which the latter is inserted in our Supplement, complete the second part: and the third part contains the history of the Society consisting of the following articles. 1. Of the Diurnal Variations of the Barometer, by Mr. Playfair. 2. Aurora Borealis observed in Day-Light, by the Rev. D. Patrick Graham. 3. Phenomenon of Two Rain-Bows intersecting one another, by Mr. Playfair. 4. On the Combustion of the Diamond, by Sir George Mackenzie, Bart. 5. Remarks on the Basalts of the Coast of Antrim, by the Rev. Dr. Richardson. 6. Rule for reducing a Square Root by a continued Fraction, by James Ivory, Esq. 7. Singular Variety of Hernia, by Mr. Russel. 8. Concerning the Chartreuse of Perth, by the Abbé Mann. 9. Explanation of the Old Word Skull or Skoll, by the Rev. Dr. Jamieson. 10. Biographical Account of the late Dr. James Hutton, by Mr. Playfair. 11. Minutes of the Life and Character of Dr. Joseph Black, by Dr. Ferguson. 12. Appendix List of Members elected since the Publication of the last Volume. 13. List of Donations.

Academical Institutions in America.

THREE Institutions for the promotion of Natural Philosophy and the Arts, having been established in the united states of America, not many months ago, of which no notice has hitherto appeared in this work, it is hoped the following account of them will not be unacceptable.

The first is an Academy of the Fine Arts, of which the first idea is due to Mr. Livingston: The public were so sensible of its importanee, that long before the arrival of the plaister

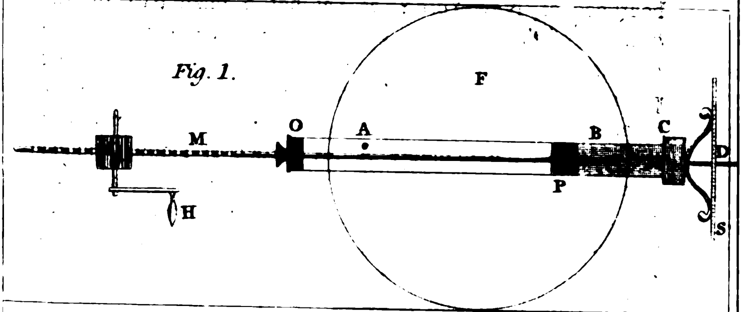
plaister of Paris casts, which he presented to the infant Society, the number of subscribers, at 25 piasters each, amounted to 180.

The second Institution is a Botanic Garden in the neighbourhood of New-York; as yet but a small part of the treasures of the vegetable kingdom are to be seen in it, but the admirers of botany hasten to send to it every interesting plant which is to be found in their vicinity. The charter of incorporation of the subscribers, is entirely conformable to the views of the founders of this garden of plants, and according to custom, ensures the permanency of the establishment: when the hot houses are finished, it is expected, that the collection of every thing rare and most interesting, produced by the southern states, will be compleated.

The third Institution is an agricultural society, established at Washington, under the special protection of government. The president of the United States, who is a most enlightened agriculturist, the chief men of the administration, the senators, and the deputies of congress, are all members of it officially. The society being now wealthy from the sums granted by government, and the numerous subscriptions of associates and correspondents, have purchased an handsome house, and a farm of thirty acres; they have also began a library; and are in possession of the fine collection of ploughs, and other instruments of Agriculture, which formerly belonged to general Washington: the form of its administration, the number and the succession of its members, the capital which it may possess (specified in bushels of corn) and its whole organization is regulated by its charter of incorporation; which constitutes this association a body politic, and fixes the perpetuity of its continuation: It is reported, that the answers which it returned to the numerous questions proposed by the societies of the different states soon after its establishment, will form a very interesting work which will soon be published.

Mr. Wright's new Air Pump.

Fig. 1.



Mr. Cuthbertson's Valve for a Condenser &c.

Fig. 6.

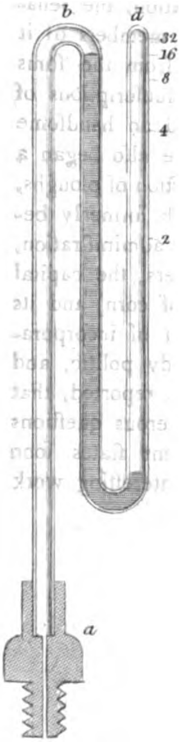


Fig. 2.

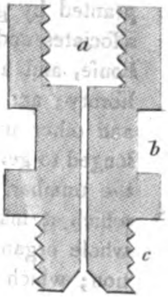


Fig. 4.

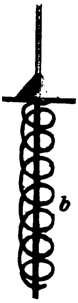
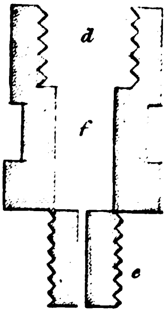


Fig. 5.

Fig. 3.



Criterion of Life.

DR. STRUVE has contrived an apparatus which is mentioned in the foreign Journals, but not described. The object of its application, is to shew by means of galvanifm, whether the appearance of death be real; a purpofe fufficiently interefting to every human being, who has for a moment reflected on the fatisfaction which recovery from apparent death muft give to the friends and relatives of the individual fuppofed to be dead; and on the ftill more impreffive and dreadful incident of recovery after burial. Our galvanic and anatomical philofophers will find no difficulty in applying this powerful agent to fo good a purpofe, in which the learned Doctor has the merit of taking the lead. Criterion of Life.

Mr. SESSKEN who has fuccefsfully laboured in the conftruction of a reflecting telefcope of thirteen feet focus, has lately fupplied the Obfervatory at Lilienthal, with two mirrors of fifteen feet focus and eleven inches aperture, which prove to be excellent, and bear the magnifying power of 2000 very well, on the proper objects, and at fuch feafons as are fit for making obfervations of this nature,

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Numbering of Houfes.

A NEW mode of numbering houfes has lately been adopted at Paris, which is attended with much advantage, and deferves to be followed in this country. Over each door the numbers are painted in large diftinct characters, and in confpicuous colours; they are generally either brown or red, on a yellow ground, furrounded by a blue fquare; but the principal fingularity, which is the object of this notice, is, that all the odd numbers are placed at one fide of the ftreet, and the even numbers at the other; by which means, may be feen at once on entering the ftreet, at which fide of the way the houfe is, which is fought for; by which much time may be faved, not only by its making it unneceffary ever to crofs the ftreet more than once, but alfo by its always Numbering of houfes.

preventing

preventing the trouble of returning back again on the other ſide of the ſtreet from that already paſſed, to find a particular number, which often happens, where the old method of numbering is uſed, from the order of the numbers proceeding regularly down one ſide of the ſtreet, and back again in the reverſed direction on the other; and which, when the ſtreets are very long, as many are in this metropolis, is often attended with ſerious inconvenience; but in the new method of numbering, this can never occur, as in it the numbers proceed in the order of progreſſion in the ſame direction at both ſides of the ſtreet.

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THE END OF THE TWELFTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar.